

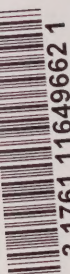
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ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY
ARISING FROM THE USE OF ASBESTOS IN ONTARIO

CHAIRMAN: J. STEFAN DUPRE, Ph.D.

COMMISSIONERS: J. FRASER MUSTARD, M.C.


ROBERT UFFEN, Ph.D., P. Eng., F.R.S.C.

COUNSEL: JOHN I. LASKIN

APPEARANCES: T. HARDY, Asbestos Information Association
of North America
M. CASGRAIN, Quebec Asbestos Mining Association
L. JOLLEY, Ontario Federation of Labour
J. McNAMEE, Government of Ontario

180 Dundas Street
Toronto, Ontario
Tuesday,
July 28, 1981

Volume XXIV



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180 Dundas Street
Toronto, Ontario
Tuesday,
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Volume XXIV

THE FURTHER PROCEEDINGS OF THIS INQUIRY
RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Perhaps we should come to order.

MR. LASKIN: Sure, Mr. Chairman.

DR. DUPRE: Counsel, any points you wish to raise
before I greet the witness?

MR. LASKIN: I don't believe so, Mr. Chairman.

DR. DUPRE: Do your colleagues have any points?

No?

Well, Dr. Finkelstein, may I welcome you most
warmly for agreeing to give sworn testimony as an expert witness
this morning.

Miss Kahn, would you swear in the witness, please?

MURRAY MARTIN FINKELSTEIN, AFFIRMED

EXAMINATION-IN-CHIEF BY MR. LASKIN

Q. Dr. Finkelstein, just for the record and
looking through your curriculum vitae, I note that you have
a doctorate in physics from Case Western Reserve University?

A. That's right.

Q. Following that, you got your medical degree
from McGill?

A. Yes.

Q. Then came to Toronto, and what is your present position?

A. I am currently employed as a medical consultant with the Ontario Ministry of Labour.

Q. In giving your testimony today, although you are employed by the Ministry of Labour, I take it you are testifying on your own behalf as an expert, and not necessarily on behalf of the Ministry?

A. Yes, that's correct. Every opinion I express will be my own personal opinion, which may or may not be shared by others in the government.

Along those lines I would also like to add that I, as part of my job description or part of my duties, am not involved in any way with the formulation of government policy. I am asked occasionally, from time to time, to express an opinion or to provide some technical advice to those who do formulate policy, but since I am not directly involved with that formulation I will not be in a position to discuss government policy here today.

Q. Has your focus of study since you have been a consultant in the Ministry of Labour been in the area of asbestos-related diseases?

A. My research has been in the area of asbestos-related diseases. I am also involved with matters of radiation protection, and I've done a review of the question of toxicity of noise exposures with respect to the Ministry's noise regulations.

Q. Good. And I take it that there are three matters that you are going to address today, the first being your general Workmen's Compensation Board study which is at tab three of the exhibit book in front of you. Secondly, your morbidity study at the Johns-Manville plant, and thirdly, your mortality study at that plant?

A. Yes, that's correct.

Q. All right. Which one do you wish to start with?

A. Well, I have actually prepared some remarks.

5 Perhaps we could start off with the remarks dealing with the first one, which was the Compensation Board study.

Q. Good.

A. If either you or the Commissioners wish to interrupt at any point, I would be quite pleased to answer any questions that come up.

10 The Compensation Board study was actually the first thing that we looked at. The aim of this study was to determine the prognosis for workers receiving compensation awards for asbestosis in Ontario.

15 The first award for asbestosis was granted by the Compensation Board in 1942, and from that date until mid-1979, there were a total of a hundred and seventy-two workers who received compensation for asbestosis.

20 The numbers that I have just given you will probably differ somewhat from those that the Board will supply you, because of slightly different classification criteria.

25 In my work I considered only those workers who received compensation solely on the basis of pulmonary asbestosis. The Board's statistics often count workers receiving compensation for any asbestos-related condition, so their numbers are not infrequently somewhat larger than mine.

30 More than half of the compensated workers have been employed at the Johns-Manville plant in Scarborough, and about a third of the workers were insulation workers.

The workers in this study were followed through the end of 1979, and we found that their mortality experience was similar to that experienced by heavily-exposed workers in other parts of the world.

We found that mortality rates from cancer and

THE WITNESS: (cont'd.) respiratory disease were markedly elevated in comparison with those of the general population of Ontario. As a result of these mortality rates, the life expectancy of the group was significantly reduced, and as a group their survival five years after compensation was only about seventy percent of that expected among Ontario men.

The Commission has expressed an interest in the relationship between asbestosis and heart disease. It has been my observation in this cohort that heart disease has not been a major problem. Only thirteen percent of deaths were due to ischemic heart disease, and mortality rates were about what one might expect among Ontario males.

It is possible that one might have anticipated slightly lower heart disease rates among blue collar workers, but I believe there is little evidence for a strong interaction between asbestos exposure and death from ischemic heart disease.

Q. Can we just follow that up for a moment, and can I take you to the, I take it the table that sets that out, which is at tab three, page 261 of your article in the CMA Journal.

A. Yes.

Q. Can you, first of all, tell us, for we laymen in the audience, what ischemic heart disease is?

A. Ischemic heart disease is disease based on hardening of the arteries, in essence. Ischemia is medicalese for lack of oxygen, so most commonly laymen would call this heart attacks.

Q. Would it include something which we have come to hear accustomed to be related to asbestosis, and that is right-sided heart failure or corpulmonale?

A. No, that would not. This is essentially the most common cause of death among men, heart attack.

Q. Looking at your table two for just a moment,

Q. (con't.d) if there was a case where someone had asbestosis and ultimately died of right-sided heart failure, would we be able to find that in your table two?

5 A. There were a number of cases. There's at least one that comes to mind immediately, and that was the first Johns-Manville compensation case, who died of corpulmonale.

Off the top of my head I can't recall what it was coded to. It would probably appear either in nonmalignant respiratory disease, or be coded to asbestosis.

10 I could check for you to find out what the case actually was coded to, but I just don't remember offhand.

DR. DUPRE: Dr. Finkelstein, is corpulmonale right-sided heart failure?

THE WITNESS: For all practical purposes, yes.

15 MR. LASKIN: Q. Just staying with page 261 for a moment, when we look down at table four, and you have divided up the mortality of certain groups in an asbestos-cement factory, and do I take it that that is the Johns-Manville plant?

THE WITNESS: A. Yes.

20 Q. What about the insulation industry that you referred to. Is that one plant or a number of plants?

A. This is a description which was put in by the editor of the journal. These were insulation workers, most of whom were members of the insulation applicators trade, the group that Dr. Selikoff was actually studying. There are two locals in Ontario of the Asbestos Heat and Frost Insulation Workers, or whatever, and these are essentially those men.

25 Q. Like Dr. Selikoff's workers, would these persons be exposed to amosite asbestos?

A. Probably. I would suspect actually that most, if not all of these men, are included in Dr. Selikoff's study.

30 Q. That appear in table four?

A. The insulation workers, yes.

Q. The Canadian portion of Dr. Selikoff's study?

A. Yes.

Q. Just before we leave your discussion of your WCB study, can I take you back to the comment you made about your criteria for categorizing these people being slightly different from the Board's criteria?

A. That's only from a statistical basis. The Board is often asked how many asbestosis cases are there in Ontario, and what they do is they count...I don't know what they do...one thing they might do is to count the cards in their asbestosis file.

These cards include not only lung cancer cases, but mesothelioma cases...or not only asbestosis cases, but mesothelioma cases and lung cancer cases. So if you count all the cards in the box, you come up with a higher number than if you count only those men who had been compensated solely for asbestosis.

That's the sense in which I mean criteria were different.

Q. All right. So it's only a question...you didn't include the malignancies?

A. No. Since I was looking prospectively to see what happens to men who are compensated for asbestosis, I included only those men who were not known to have a malignancy at the time the compensation award was made.

There have been a number of asbestosis compensations made to widows on a post mortem basis, but it would obviously be inappropriate to include those men in a prospective study.

There were also a number of men who the Board has compensated solely for malignancies, mesothelioma or lung cancer. I also excluded those from the asbestosis study.

Q. Looking just for a moment again at table three on page 261, under the category of nonmalignant respiratory disease, which appears to account for twenty-three of the observed

5 Q. (cont'd.) number of deaths of sixty-one, can you tell us, without having to go back and check, can you tell us how many of those would actually have died of asbestosis and how many would not have died of asbestosis but would have been noted as dying of a nonmalignant respiratory disease?

A. I have all that information available at my office. I have none of those numbers in my head.

10 I might say that in contrast to some people, I chose not to pull out the category asbestosis separately. There is some feeling that the actual name you put on the death certificate is quite subjective. Whether you write down bronchitis, or you write down emphysema, or you write down asbestosis or silicosis, or whatever, is open to some interpretation on the part of the certifying physician.

15 I felt the best thing to do would be to lump together all the respiratory diseases and I've done that. Some of these men may have been coded as dying of chronic bronchitis or emphysema or something like that, but as I say, I've got all that data, but I can't tell you what it is right now.

20 Q. Okay.

DR. DUPRE: May I ask a few questions about these tables? I gather you just...

MR. LASKIN: You carry on, Mr. Chairman.

25 DR. DUPRE: I just want to make sure I understand what I'm looking at, Dr. Finkelstein, and maybe it will be helpful to me if we look at table two and, let's say, just concentrate on the mesothelioma line.

THE WITNESS: Yes.

30 DR. DUPRE: Now, as I take it from the footnote, the numbers that are not in parentheses are the numbers of mesothelioma deaths that were registered on death certificates, is that correct?

THE WITNESS: These are the numbers that received

THE WITNESS: (cont'd.) the ICD code either 163 or 158. There were some death certificates which said pleural mesothelioma and which received the ICD code by the coders at Queen's Park, 162. This is the code for lung cancer.

DR. DUPRE: I see.

THE WITNESS: So the numbers there are based not on verbal descriptions, but on the actual numerical codes which were applied to the death certificate by the official coders at Queen's Park.

DR. DUPRE: And that number, as coded by the official coders, is a total of three mesothelioma deaths ...

THE WITNESS: Yes.

DR. DUPRE: ...all between the ages of forty-five and fifty-four, correct?

THE WITNESS: These men were forty-five to fifty-four at the time they were compensated. They may have been a few years older at the time they died. I'm not sure.

DR. DUPRE: Now, if I add up the numbers in parentheses, these numbers represent both the number of deaths that were officially coded, plus the number of deaths where the cause was changed?

THE WITNESS: Yes, that's correct.

DR. DUPRE: So adding one, five, one and two, I get a total of nine?

THE WITNESS: Yes.

DR. DUPRE: Which I then see is faithfully replicated in the mesothelioma column in table three, correct?

THE WITNESS: Yes.

DR. DUPRE: So that exactly balances with table two. Now, if I go to table four, there I find that in terms of the expanded total, seven of the nine mesothelioma deaths occurred among the asbestos-cement workers in that single asbestos-cement factory in Scarborough?

THE WITNESS: Yes.

DR. DUPRE: The J-M plant, correct?

5 And then when I go to the righthand side of table four, I find that the other two mesothelioma deaths occurred among the insulation workers.

THE WITNESS: Yes, that's correct.

10 DR. DUPRE: Is that total of nine mesothelioma deaths the sum total of mesothelioma deaths that have been compensated by the WCB up to 1979?

THE WITNESS: No. No, it's not. I think there were twenty-five up until the time frame of this study. I have recently been informed their totals are now somewhere around fifty or fifty-one.

15 These nine deaths are solely the deaths occurring among men who were first compensated for asbestosis.

DR. DUPRE: Okay.

THE WITNESS: Most of the other mesothelioma deaths occurred in men who may or may not have had asbestosis, but who either had cancer or had died of cancer.

20 DR. DUPRE: Okay.

MR. LASKIN: Q. So these are only the deaths where there was pre-existing compensable asbestosis?

THE WITNESS: A. Correct.

25 Q. If you ask the same question with respect to lung cancer, do you also find that there were lung cancers compensated where there was not pre-existing compensable asbestosis?

A. The best people to ask will be the Compensation Board people. I suspect that that is true. I can't document that.

30 There...although there certainly have been Johns-Manville workers compensated for lung cancer who did not receive compensation for asbestosis, so I guess that is definitely true.

Q. One other question, and Dr. Mustard has a question, but could you...without getting too complicated...could you just tell us generally how you came to the conclusion that the survival rate of this group of people was about seventy percent of the normal five years after compensation? How do you make that calculation and how do you get to that?

A. I believe it's described in the paper that was in the appendix to the brief. Basically what I did was, for each compensated man I selected a hypothetical control of the same age.

Q. One control?

A. One control. The vital statistics office of Ontario, or Statistics Canada, publishes tables which show you in any given year the probability of a man of age fifty, or whatever, of surviving to reach the next age. So I had a group of a hundred and fifty-seven, or whatever, asbestotics. I had a hypothetical group of a hundred and fifty-seven makebelieve men of the same age. From these Ontario tables I was able to calculate how many hypothetical men would be alive five or ten years after the starting point.

Okay?

I then looked at how many of the compensated workers were alive five or ten years after the starting point, and just took the ratio, which was seventy percent at five years, and about fifty-three percent at ten years.

DR. UFFEN: I'm going to ask a somewhat general question so I don't get to listen for an hour wondering whether I'm lost.

I'm comparing what we call tab three with what I've got, I think it's tab five. One comes from Workmen's Compensation records, and the other one we haven't yet talked about, but it's something else?

THE WITNESS: Yes, that's correct.

DR. UFFEN: The number of mesotheliomas in tab three comes to nine. In the other one, it's six, it's less than that. It seems to me from what I read it's six.

5 Either I've made a mistake...I don't know how we could have lost three dead...

THE WITNESS: No, the ones that come to six in tab five are the coded mesotheliomas, so you have to compare that with the number three in table two here.

10 DR. UFFEN: Which is best available data?

THE WITNESS: No, they are compared coded with coded, so table two in this paper we are now discussing shows that there were...well, okay. Table four, you look at the number which is not in brackets, and that is two. That should be compared with the six that appears in tab five.

15 MR. LASKIN: Q. It's seven, actually, isn't it? In tab five?

THE WITNESS: A. Whatever.

DR. MUSTARD: It's thirteen in my calculation, just to add to the confusion, in table one.

20 THE WITNESS: The important point is to compare like with like - coded versus best evidence.

DR. UFFEN: Would it be just...having noticed some confusion, could you be careful on the way through to...

MR. LASKIN: Seven.

25 THE WITNESS: Yeah, okay, but this is the sum of those two.

DR. UFFEN: Now that you know that we might be a little confused, could you be a little bit careful so that we can bear in mind which it is you are talking about each time?

THE WITNESS: I will try to be careful. Interrupt me if I am confusing you, and I will attempt to disconfuse you.

30 DR. DUPRE: Maybe if counsel would permit, I

DR. DUPRE: (cont'd.) could ask Dr. Finkelstein a question comparing tab three and tab five, that will at least set a couple of reference points straight in my mind.

Now, as I understand it, Dr. Finkelstein, the lefthand side of table four in tab three...

THE WITNESS: Yes.

DR. DUPRE: ...refers to the same plant as is studied in tab five.

THE WITNESS: That's right.

DR. DUPRE: However, the population of workers that is treated on the lefthand side of table four in tab three, is only that population from that plant who were certified asbestosis patients?

THE WITNESS: Absolutely correct.

DR. DUPRE: Whereas on the other hand, the population from that same plant that is treated in table one involves all deaths that occurred among all workers in that same plant?

THE WITNESS: That's right.

Actually, if we are talking about mesothelioma at this plant, Mr. Laskin has a handout, actually, in which I tabulate all the mesothelioma deaths. Perhaps you could distribute that at this point.

MR. LASKIN: Let's clear up the confusion so that we all know where we are going, and I'll get to that in a moment. But let's just compare...let's go to your table one in tab five, and I think we can clear up all the confusion.... which is right near the end.

The figures on table one are figures taken from the death certificates?

THE WITNESS: A. Yes.

Q. At Johns-Manville?

A. Yes.

Q. Regardless of whether or not there was pre-existing compensation for asbestosis?

A. Yes.

5 Q. All right. As I read this table, because you add up the second and third columns to get six, the death certificates show seven mesotheliomas?

A. Yes.

10 Q. All right. Now, the best evidence...because you made a best evidence calculation in tab five...in your paper you have the figure thirteen...is it thirteen or eleven?

A. It's actually ten.

Q. Well, if we...all right, if we go to...

DR. UFFEN: Table two says eleven.

15 THE WITNESS: A. I think the best way to approach this would be in sequence, rather than...

MR. LASKIN: Q. I think that's right, but the point is that that's where the difference in figures comes in?

THE WITNESS: A. Yes.

20 Q. All right. I think it's probably easier if we do it in sequence, and we will come to the mesotheliomas in the mortality study and the figures are slightly changed, I take it, now, Dr. Finkelstein?

A. Yes.

DR. MUSTARD: Can I ask a question then, back to tab three?

25 MR. LASKIN: Yes.

DR. MUSTARD: Table two on page 261 in tab three, I think if my calculations are correct, indicates that a number of deaths were reclassified as mesothelioma if one takes the figures in brackets?

THE WITNESS: Yes.

30 DR. MUSTARD: I would have surmised that the bulk of those reclassifications might have been from the lung cancer/

DR. MUSTARD: (cont'd.) gastrointestinal cancer group. If I look at the reclassifications in those two groups, it doesn't provide me with an explanation as to where some of the mesotheliomas have come from.

Do you have any clue as to what...?

THE WITNESS: Yes. One man...well, that's because the lung cancers were also increased when I looked at the best evidence. One man who died at Sunnybrook Hospital with lung cancer metastatic to his brain was coded as dying of a heart attack, or heart failure.

There was another case of a man who, on his death certificate, was listed as asbestosis, and then lung cancer, and this was coded to asbestosis.

So I've revised not only the mesothelioma column, but the other categories as well. So some of the mesotheliomas came from the lung cancer, but the lung cancers themselves were augmented from other categories.

DR. MUSTARD: Any from the gastrointestinal cancer area moved to mesothelioma?

THE WITNESS: I think not, but I can't say with one hundred percent certainty.

DR. DUPRE: One other question on table two on page 261, Dr. Finkelstein. This is by age at time compensations were awarded?

THE WITNESS: Yes.

DR. DUPRE: I should read that as meaning the age at time a partial disability or full disability pension was awarded?

THE WITNESS: I considered only those people given a partial disability rating. People with so-called medical aid or men classified as having asbestosis but not disabled were not included in this study. These are men who have received a pension from the Board.

5 DR. DUPRE: Insofar as some of these men may have started with a pension calculated at a certain level of disability, but then received a higher level subsequently, the age that is critical here is simply the age at which **they** received their very first pension?

THE WITNESS: That's correct.

10 DR. DUPRE: If I remember rightly, you comment on somewhere in this paper, but I cannot put my hand on it offhand, the number of years that separate the initial granting of a disability pension and the time of death.

THE WITNESS: I can't think of what you have in mind.

15 DR. DUPRE: Well, for example, just in terms of where you are going at the bottom of page 261 on your way to the top of 262, you have calculated a curve showing survival rates...

THE WITNESS: Yes.

DR. DUPRE: ...for those with asbestosis...

THE WITNESS: Yes.

20 DR. DUPRE: ...with sixty-nine percent at five years, and fifty-three percent at ten years, from the date that compensation was awarded?

THE WITNESS: Yes. The actual graph appears in the appendix...

DR. DUPRE: Right.

THE WITNESS: The proof setters chose not to include it on this galley proof.

25 DR. DUPRE: That's not the figure on page 108 of the typed script, that is tied to the preliminary...and becomes part of your brief?

THE WITNESS: Yes, it is.

30 DR. DUPRE: On your way to making these calculations, you collected data case by case on the number of years that separated the granting of the first disability pension and the

DR. DUPRE: (cont'd.) time of death, if death occurs?

THE WITNESS: Yes, that's right.

5 MR. LASKIN: Q. Can we move on to your two studies specifically at the Johns-Manville plant, and can you tell us firstly what the purpose of your studies was?

THE WITNESS: A. We had two purposes in mind. Firstly, we were interested in discovering if there were any former workers who had developed conditions which would entitle
10 them to compensation awards, but who for one reason or another had not filed claims with the Board, or their surviving family members had not filed claims, so the first aim, essentially, was case finding for compensation purposes.

Secondly, we were interested in scientific issues, investigating exposure-response relationships for disease among
15 workers at the factory.

Q. In respect of the first purpose, was that with the blessing of the Board? In other words, what...

A. The Board has been kept informed of what we are up to. We also actually required permission from a third
20 party, who is the Deputy Registrar General for Ontario.

Since the deaths that we were encountering, many of them came from death certificate records which are confidential documents, we required the approval of the Deputy Registrar in order to release this information to the Board.

Their procedure, once I informed them of a
25 possibly compensable case, is to attempt to locate the widow or next of kin and to inquire as to whether or not they wished to establish a claim.

Apparently in some cases, not necessarily in asbestos cases, but in the Board's experience there have been
30 widows who have not been interested in establishing claims.

So that's our procedure. We ascertain a death,

A. (cont'd.) we seek approval from the registrar to release this confidential information, and we then release it to the Board.

5 The registrar has been very understanding and very co-operative, and has not denied us approval on any case.

Q. Now, before you get into the details of your study, can we just understand what was being made, what was being produced, at the plant out in Scarborough? As I read your papers, there appear to have been four different products being
10 produced - number one, asbestos-cement pipe, which, as I read your papers, was produced from 1948 onwards?

A. Yes.

Q. And according to your materials, was made from both chrysotile and crocidolite?

15 A. Yes, that's correct. There apparently was some question whether amosite had ever been used, and I think the company has informed me that it has not been used or was not used in the pipe operations. It was just chrysotile and crocidolite.

20 Q. Is that information that you have just told us about fiber types, has that information come from the company itself?

A. Yes, it does. Although I imagine the workers as well know what they were using. The fibers look different, they talk about white and blue. Amosite is brown. So they themselves would have some idea of what they were working with.

25 Q. To your knowledge is the percentage of crocidolite in the final product roughly about what we have heard with respect to other asbestos-cement pipe operations? That is, somewhere around three percent?

30 A. Apparently it varies on the diameter of the pipe that they are making. Three percent is a ballpark figure.

Q. Then secondly, as I understand it, also

Q. (cont'd.) beginning in 1948, this plant produced rockwool insulation?

A. Yes.

Q. And that comprised essentially your control group of minimal or no asbestos exposure?

A. Yes, there were a few additional people from other areas of the plant. This was the control group, yes.

Q. Then thirdly, from 1955 to 1970, as I read your papers, this plant produced asbestos-cement board using only chrysotile?

A. Yes. With cement and silica as well, but chrysotile was the only asbestos fiber.

Q. Then finally, from 1960, this plant also made asbestos insulation materials?

A. Yes...a product called Thermobestos, which was made from amosite asbestos. They, in recent years, have substituted fiber glass, I believe, for the asbestos. I can't tell you what date this changeover was made.

There was also another product there. They were involved with brake linings, I believe, in one area of the plant. None of the workers in the group that I have studied so far were involved in that operation.

Q. In respect of the insulation materials, and bearing in mind the criteria for entry into your various cohorts, were any of the employees in the asbestos insulation end of the business included in either your morbidity or mortality cohorts?

A. There were two or three who had spent brief periods of time after 1960 in the Thermobestos area.

Q. So do I take it that as far as your asbestos-exposed workers are concerned, they essentially came from A-C pipe and A-C board operations?

A. Yes.

Q. Is there physically, at the plant was there

Q. (cont'd.) separations between buildings?

A. Yes, there were two buildings.

Q. Two buildings? Was there any interchange of
5 employees as between those two operations?

A. Yes, there were. From the employment histories,
it appears that when they opened the board plant in 1955, they
hired essentially a fresh work force to staff that shop. However,
there were men who had worked in the pipe plant who did go to work
in the board shop, and some of the men who worked in the board shop
10 subsequently worked in the pipe plant as well. There was movement
between the two areas.

Q. In your own calculations, have you made an
allocation as between who worked where and when?

A. I added up...well, I determined exposures for
various jobs in both shops. I just added them up to give the
15 grant total at the end.

It will be interesting at some point to look at
those who were exposed only to chrysotile in the board shop.
However, since this shop only opened in July, 1955, latent
interval problem means that there just hasn't been enough time to
20 look at this group separately. so all the asbestos-exposed
workers are combined.

Q. All right.

DR. DUPRE: Can I just ask a question here?

You talked about the board shop and about the
pipe shop. Now, where was the Thermobestos insulation material...
25

THE WITNESS: There was another shop as well.

DR. DUPRE: That was a third shop?

THE WITNESS: I think there were actually four.
There may have been more than that. But the rockwool, Thermobestos,
flex board and pipe were in four sheds. I'm not quite sure.
30 There are more buildings on the site. I'm not quite sure what
went on in the others, but these were four separate areas.

DR. DUPRE: The workers in those extra shops or sheds are included in tab five?

5 THE WITNESS: I've got three groups of workers in that study - the asbestos-exposed workers who worked in the pipe and board shops, the maintenance workers who worked all over and were exposed to asbestos in the pipe and board shops, and quite probably in the Thermobestos plant as well after 1960, and the control group who were the rockwool/fiber glass workers, guys who worked for less than twelve months in either the pipe
10 or board shops, a couple of guys who worked at the gatehouse, some people who worked in the power house, the majority of the control workers were the rockwool/fiber glass, but there were a few from other areas of the plant as well.

15 DR. DUPRE: I'm not quite sure I followed that. Where are the Thermobestos people involved?

THE WITNESS: Basically they don't come in because the way the hiring patterns were, since I looked only at people hired to 1960, only two or three of the men in my study ever worked in the Thermobestos shop.

20 As I extend the study to later intervals of time, then men working in the Thermobestos shop will enter the cohort.

DR. DUPRE: So nobody from...nobody who acquired his initial employment in the Thermobestos shop is in this study?

THE WITNESS: That's right.

25 DR. DUPRE: Because the cutoff date is after your study?

THE WITNESS: That's correct.

DR. DUPRE: Insofar as there are any people who were in the Thermobestos shop who are in your study, these are individuals whose initial exposure was in other shops...

THE WITNESS: Correct.

30 DR. DUPRE: ...and who moved to the Thermobestos shop?

THE WITNESS: A. Yes. And again, there were possibly three, possibly two, I can't recall offhand which of those numbers is correct, but there were certainly no more than three men in my cohort who did work in the Thermobestos area.

MR. LASKIN: Q. Without taking you...I don't want to take you away from your comments too much, but just looking at your page six of tab four, for a moment...

THE WITNESS: A. Yes.

Q. Looking just at the first paragraph under 'results'. First of all, I take it criteria for entry into your cohort was a minimum employment of fifteen years?

A. Yes, and the hiring before 1960.

Q. And hiring before 1960.

I note that not all of the hundred and fifty-seven production workers who were asbestos-exposed were in fact asbestos-exposed for all the fifteen years, and I'm just...where were they? Were they in other areas of the plant?

A. Yes. They worked in rockwool/fiber glass, they worked out in the yard driving trucks, or they came close to retirement age and worked at the gatehouse doing security things.

Q. Just before you go on...

A. I might add that, you know, I mentioned the criteria of fifteen years of employment and hired before 1960, the other criterion was a minimum of twelve months of exposure in asbestos areas. Some of these men may have spent a month in the pipe plant and all the rest of their time in rockwool/fiber glass, or they might have spent two years in the pipe plant and the rest of their time in the fiber glass operation.

Q. So we have those three criteria for entry into your cohort- that is, a minimum fifteen years employment, minimum twelve months exposure to asbestos, hired prior to 1960?

A. Yes.

Q. But in fact, of a hundred and fifty-seven production workers that you concentrated on, four-fifths of them, eighty percent in fact had fifteen years exposure to asbestos, at least?

A. Yes.

Q. Before you go on, can you just clear up for me, anyway, one other term which you use in your paper, and that is what you refer to as mixed dust pneumoconiosis as determined by the advisory board when you come to deal with your statistics. Is there some difference between that phrase and asbestosis?

A. It's a matter of terminology. The advisory committee is a group which essentially examines the men coming to the Board, and it advises the Board on medical matters.

They realize that these people have been exposed to asbestos and to silica. In order to be academically correct, they like to call these men 'mixed-dust pneumoconiotics'.

The Compensation Board calls them asbestotics.

They are the same people, it's just a question of labels.

So that's how those terms arise.

Q. All right. Then dealing with your...

MR. LASKIN: Did you have a question, Mr. Chairman?

DR. DUPRE: Maybe I should just express the sentiment like my colleague on my right expressed awhile back, I feel a little bit at sea in terms of the population that is studied on tab four, in relation to the populations that are studied under tab five.

It may not be the time to clear that up.

MR. LASKIN: I think we can clear it up very quickly.

As I understand it, and you correct me...you tell me if I've got it wrong, Dr. Finkelstein...we discussed your criteria for entry into your cohort for your morbidity study - that is fifteen years minimum employment, hiring before 1960, minimum

MR. LASKIN: (cont'd.) twelve months exposure to asbestos...

5 DR. DUPRE: State that slowly, counsel, so I can write it down.

MR. LASKIN: Three criteria...
Have I got them right, first of all, Dr. Finkelstein?

THE WITNESS: Yes, it sounds pretty good to me.

10 MR. LASKIN: All right.

DR. DUPRE: One.

MR. LASKIN: Minimum fifteen years employment at Johns-Manville.

Two, minimum exposure to asbestos of twelve months.

Three, must be hired before 1960.

15 THE WITNESS: Correct.

MR. LASKIN: All right.

MR. LASKIN: Q. When you take those three criteria and look at all the employees in Johns-Manville, I take it you get a hundred and fifty-seven production workers, and forty-four maintenance workers?

20 THE WITNESS: A. Yes.

Q. For the purpose of your more intensive studies, you have chosen not to include the maintenance workers?

A. For the purpose of my quantitative studies I have chosen not to include them.

25 Q. Just stopping there for a moment, we'll clear up the difference, but can you just tell the Commission briefly why you have made that decision for your quantitative studies not to look at the maintenance workers?

30 A. I didn't see any way in which I could assign a quantitative exposure to maintenance workers. This is in contrast to both Drs. Enterline and Weill, who felt they could.

A. (cont'd.) I, quite frankly, don't see how they could do that. As a matter of fact, they actually didn't do it. The companies did it for them.

5 So this was my decision based on my examination of the raw data.

10 It's well known that maintenance workers are here today, there tomorrow, and someplace else the next day. Given that the sampling was done no more frequently than once every three months, since the employment histories didn't indicate what job a maintenance worker was doing on any given day, they just said that he was a millwright or an electrician, I felt it was absolutely impossible to make any quantitative exposure assessment.

15 Q. Then just one final question on the criteria for entry to your morbidity cohort. Is there any magic in fifteen years? Why fifteen years?

20 A. Purely arbitrary. It was based on the observation that most of the men with less than fifteen years of employment had disappeared from view. Since in the morbidity study we were concerned with men who had come to the attention of the Compensation Board, I was faced with the choice of introducing one of two biases. I could include everybody and byso doing include people who might have developed asbestosis, but not come to the Compensation Board's attention, in which case I would have underestimated the risk of asbestosis, and I could have made the other choice, which I did, which was to exclude the shorter-term people and risk overestimating the risk at the lower exposure end.

25 It was a choice of...

Q. It was a judgement you made.

30 A. Yes, it was a judgement. Whatever I did, I would introduce an error. I chose to select the approach that I did.

Q. We may elaborate on that when we come to the more specific results, but just in comparing that cohort with your mortality cohort, now have I got this right...when you look at your mortality cohort, your criteria for entry are, number one, minimum nine years employment as opposed to fifteen?

A. Yes.

Q. Second, minimum twelve months exposure to asbestos?

A. To enter one of the asbestos-exposed groups. Otherwise, you are a control.

Q. Yes, all right. In other words, in your mortality study you have got three exposure groups as I recall it, either zero to sixty-nine fibers per c.c. years, sixty-nine to one twenty-one, and one twenty-two to four twenty...if I remember the figures correctly...and those are your three exposure groups, and to come into one of those groups you have to at least be exposed for one year?

A. Yes.

Q. All right.

Then the third criteria is hiring before 1960?

A. Yes.

Q. So that when you strip it all aside, the real difference between your morbidity cohort and your mortality cohort is that in the former you require a minimum of fifteen years employment, and in the latter you require only a minimum of nine years employment?

A. Yes.

Q. When you get your mortality cohort, you've got a hundred and eighty-six production workers, who I take it would include the hundred and fifty-seven production workers in the morbidity cohort?

A. Yes, absolutely. I might add, the mortality study will be, when completed, looking at everyone with twelve months of employment. I've divided them into two groups - those

A. (cont'd.) with nine or more years of employment, because I could trace them more readily...

Q. Because of your tracing problem.

A. Yes, I am currently tracing the other group, so eventually it will be everyone with a minimum of one year employment.

Q. Just to clarify the last point, your hundred and fifty-seven production workers with a minimum of fifteen years employment are part of the hundred and eighty-six production workers in the mortality study?

A. Yes.

Q. So that the additional twenty-nine would have been employed somewhere between nine and...

A. And fourteen years, yes.

Q. ...and fourteen years. That's right, somewhere between nine and fourteen years. All right.

DR. DUPRE: Do I take it that the third criterion for the tab four calculation, must be hired before 1960, does not apply to the tab five list?

THE WITNESS: No, it does.

DR. DUPRE: It does?

MR. LASKIN: Yes, it does.

DR. DUPRE: Okay.

DR. UFFEN: Including the maintenance workers, who come and go?

THE WITNESS: Anyone considered in any of the analyses before you was hired prior to 1960.

MR. LASKIN: Q. Just on the maintenance workers, in your mortality cohort you've got fifty-five maintenance workers, in your morbidity cohort, forty-four. So the additional eleven were persons employed before 1960, but for somewhere between nine and fourteen years?

A. Yes, that's right.

DR. DUPRE: I think I've got it. Just to make absolutely certain, let me try this out on you, Dr. Finkelstein.

5 I will say that the tab four population was selected in accordance with the fifteen-twelve-1960 formula: fifteen years, twelve months and prior to 1960?

THE WITNESS: Yes, that's right.

10 DR. DUPRE: The only difference is that the tab five population was selected in accordance with what I will call the nine-twelve-1960 formula. The only difference, therefore, between the tab four population and the tab five population is the number of years of employment?

THE WITNESS: No, not true. The twelve enters in only partially in tab five. The twelve months of exposure applies to my asbestos-exposed subgroup.

15 If they had less than twelve months of exposure, they enter the control subgroup.

MR. LASKIN: If one looks at table one in tab five...it's taking you far out of your train, but...I take it what you have done for comparison purposes is that you have established an internal control group within the plant?

20 THE WITNESS: Yes.

MR. LASKIN: Q. And that's the rock insulation people, essentially?

THE WITNESS: A. Yes.

25 Q. All right. Those rock insulation people form part of your study in tab five, they are your control group, and they would not have to be exposed to asbestos, indeed you would hope that they wouldn't be exposed to asbestos at all for your control purposes?

A. Some of them may have been exposed for up to, but not greater than, twelve months.

30 Q. So are we now clear on that? The production workers...

DR. DUPRE: I'm sorry.

MR. LASKIN: Well, let me see if I can try it and you tell me if I'm wrong, Dr. Finkelstein.

THE WITNESS: May I suggest that we get to this a little bit later and...

DR. DUPRE: Well, I think it will help the Commission if they understand just who you are comparing, and I think I can do it briefly.

THE WITNESS: Okay. Well, let's look at table one of this tab, the last tab.

MR. LASKIN: Q. You've got three categories of employees, isn't that right?

THE WITNESS: A. Yes.

Q. You've got production workers, maintenance workers and a control group?

A. Yes.

Q. Your production workers and your maintenance workers are asbestos-exposed employees either in A-C pipe or A-C board?

A. Yes.

Q. All right. Your control group are rock insulation workers who fit two of the three criteria for your asbestos-exposed persons - that is, they were employed before 1960, and a minimum employment of nine years.

The only criteria they don't fit is the third one - they weren't exposed to asbestos.

A. For more than twelve months.

Q. For more than twelve months.

A. Yes.

DR. UFFEN: There's one other criterion we must have down there - as defined by the Workmen's Compensation data. This applies to them all, doesn't it?

THE WITNESS: No, no. We finished with the

THE WITNESS: (cont'd.) Workmen's Compensation data.

DR. UFFEN: So these figures don't have to agree with the tab three figures?

THE WITNESS: Correct.

DR. UFFEN: Except one part of tab three which included...

THE WITNESS: All this categorization was defined by the company's employment records, which I might add, are superb. They list on a chronological basis every job that the worker did, so one can tell just by looking at these pieces of paper who worked where and when.

DR. UFFEN: Thanks, counsel.

It's probably obvious that we are not just trying to be difficult. This is a small number and little differences in a small number of cases may have quite a bit of significance.

THE WITNESS: Oh, absolutely.

DR. UFFEN: So we better not stay confused.

DR. DUPRE: And as I take it, to make sure I always have it straight, tab three is fundamentally different from tab four and five because the criterion for entry into tab three...

THE WITNESS: Is compensation.

DR. DUPRE: ...is certified asbestosis as defined by the WCB.

THE WITNESS: Correct.

MR. LASKIN: Well, not to get...

MR. HARDY: Mr. Chairman, on the criteria there is one question I'm not clear on, and if I could just ask it here, it would be within context.

In order for the maintenance workers to be included in the exposed group, they, too, I gather, would have had to have at least twelve months exposure to asbestos?

THE WITNESS: Yes.

MR. HARDY: And you've already told us it's difficult to know how much exposure the maintenance workers had because they worked in different places around the plant. How did you determine that they had twelve months exposure?

THE WITNESS: I based it on their job title.

MR. HARDY: Could you elaborate on that a little?

THE WITNESS: If a man was described as a millwright, I assumed he worked in asbestos exposure. He may have had no asbestos exposure whatsoever, in which case the apparent risk in the maintenance workers is underestimated.

MR. HARDY: Are there some types of maintenance workers at the J-M plant from whose job descriptions you concluded they would not have been exposed to asbestos?

THE WITNESS: Yes, the machinists, for example, people who worked in shops where people brought in something for them to fix and they didn't actually go out.

MR. HARDY: So primarily you included maintenance workers who travel around the plant?

THE WITNESS: Yes.

MR. HARDY: But not maintenance workers who worked in a fixed point in the plant?

THE WITNESS: If the fixed point in the plant happened to be next to a pipe rolling machine.

MR. LASKIN: O. Okay. I just want to clear up, and I don't want to confuse the record any more, but I just want to clear up one matter arising out of the Chairman's last question when he said that fundamentally tabs four and five are different than tab three.

I do take it from reading tab four that one of the calculations you have made in tab four is the mortality of the J-M workers who were certified for compensation.

In other words, you have taken a subset...

THE WITNESS: A. Yes.

Q. ...of some of the figures you've got in your tab three, your more general paper, and done it for the J-M plant?

A. Yes, that's right.

5 Q. Okay. That appears at table one at tab four, and I just wanted to make sure you understand that.

I'm sorry, Dr. Finkelstein, but maybe it might..so that we are all clear...but if one goes to table one of tab four...

DR. DUPRE: Table one, tab four.

10 MR. LASKIN: Q. We have in table one at tab four, sixty-one persons, as I read it, who are certified compensable for asbestosis, and you then looked at their subsequent mortality and they added man-years of observation after they were certified, so that table one, if you look at it in tab four, is somewhat similar to your table three in tab three?

15 THE WITNESS: A. Yes.

Q. Have I got the right comparison? With a smaller subset?

A. Yes.

Q. Are we all clear on that?

20 DR. DUPRE: Are they in fact identical, because table one in tab four doesn't start with the figure sixty-one men, and table three on page 261 of tab three begins with 'observed deaths from all causes - sixty-one'?

MR. LASKIN: No, that's deaths. Sixty-one, as I read it, is people compensable.

25 THE WITNESS: Yes. Out of the men with fifteen years of employment, there were sixty-one production workers who received certification.

MR. LASKIN: Q. And of those, twenty-four died up to the end of your study?

THE WITNESS: A. Yes.

30 Q. What you then did is you did a man-years calculation and you came up with a relative risk factor?

A. Yes.

Q. Table one refers to J-M people, table three
in tab three is just Ontario, the province?

A. Yes, yes.

MR. LASKIN: Okay?

DR. DUPRE: It's purely fortuitous...

DR. MUSTARD: Just to make it clear so I don't stay
totally confused over this dialogue, table four in tab three
has asbestos-cement workers and insulation workers?

THE WITNESS: Yes.

DR. MUSTARD: Right? Table three then is a broader
composition than the information in table four? It's all people
in Ontario?

THE WITNESS: Yes, that's right. There were...
approximately fifty percent were J-M people, approximately one-third
were insulation workers, and the remainder were miscellaneous.

DR. MUSTARD: But of the sixty-one men in table
one in tab four, some of those would appear...or all of those
would appear in the calculation of table three?

THE WITNESS: Yes.

DR. MUSTARD: All would appear in the calculation
of table three, is that correct? There would be...they would be
a subset within table three?

THE WITNESS: Yes, that's correct.

DR. MUSTARD: Okay.

DR. DUPRE: Now, just to follow on that, what
about the lefthand side of table four in tab three? In relation
to table one in tab four?

THE WITNESS: Well...

MR. LASKIN: It could be persons with less than
fifteen years employment.

THE WITNESS: I would prefer to address all of this
directly, which I have done in my prepared remarks, rather than

THE WITNESS: (cont'd.) from table to table.

So if after I've told you what I have to say there are still some questions, then perhaps that would be the time...

MR. LASKIN: I'm sorry we have taken you away.

Okay, let's...tell us what you set out to do in your morbidity study.

THE WITNESS: A. Okay. There were three possible indicators of morbidity that one could look at and which we actually are in the process of looking at. The first of those is the question of x-ray abnormalities, which you have described by Drs. Becklake, Weill and others. Secondly, there are pulmonary function abnormalities, which again you have heard described, and lastly, this question of compensation for asbestosis.

Workers at the J-M plant are under medical surveillance by the Occupational Chest Disease Service of the Ministry of Labour, which examines workers with eighteen or more years of employment, every six months, and shorter-term employees are examined annually.

During these examinations by the Ministry, workers receive both a chest x-ray and pulmonary function testing.

There is thus a repository of x-rays and pulmonary function results available for epidemiological investigation.

Unfortunately, from an epidemiological point of view, the x-rays had not been coded with epidemiology in mind and the ILO coding scheme, which is the international scheme which is used for epidemiological x-ray studies, had not been used.

Dr. Jerry Vingilis of the chest service has thus reinterpreted for me the films of the long-term employees and has recoded them according to the ILO scheme.

I am currently working to correlate these x-ray findings with asbestos exposures, but this work is still in progress and is not yet completed.

THE WITNESS: (cont'd.) I also hope in the not too distant future to have a look at the pulmonary function testing results and to correlate those with the exposure data as well, but
5 unfortunately I have not yet begun that work.

MR. LASKIN: Q. Do you expect that work will be completed within the relative lifetime of this Commission?

THE WITNESS: A. The x-ray work might be, the pulmonary function will almost certainly not be completed.

Q. I refrain from asking before or after the
10 wedding.

DR. UFFEN: I think we have to introduce a term called the half-life.

MR. LASKIN: Good point, Dr. Uffen.

THE WITNESS: The easiest analysis from the view
15 of data availability, was the examination of the relationship between compensation for asbestosis, which the British call certification, and is a term I have adopted, the relationship between certification and asbestos exposure. This is described in one of the papers that I have given to you.

There are several complicating factors involved in
20 this assessment, the first of which is that it generally takes many years for clinical asbestosis to appear, and I've got a slide which illustrates this.

We don't really need the slide, because this is figure one on tab something or other.

MR. LASKIN: Tab four. Tab four, figure one.
25

THE WITNESS: This indicates the distribution of time intervals from first exposure to asbestos to certification, by the advisory committee. As you can see, the vast majority have been later than twenty years from first exposure.

Because of this delay interval, I decided to
30 include in the analysis only those workers hired prior to 1960, so that's where the 1960 figure magically appears.

THE WITNESS: (cont'd.) Secondly, most of the short-term employees had disappeared from view. By that I mean they have left employment, they were no longer under surveillance by the Ministry of Labour, and we basically had no idea what had happened to these people.

DR. MUSTARD: Could you clarify what short-term means? What are the exclusion criteria in short term?

THE WITNESS: Short term is just a vague, descriptive kind of thing.

DR. UFFEN: If I worked two years would I be short term?

THE WITNESS: Yes.

DR. UFFEN: Even if I was exposed flat out for two years...

THE WITNESS: Yes.

DR. UFFEN: ...I would still be classified as short term?

THE WITNESS: They are short term in the sense that I don't know what has happened to them yet.

DR. DUPRE: Well, by definition nobody here has been employed for less than nine years, correct?

THE WITNESS: No. I'm talking...

DR. DUPRE: I'm sorry, fifteen.

THE WITNESS: ...I have a list from the company of everyone who has ever worked there for a day or more, and I'm now describing how I came to select fifteen year figure.

DR. DUPRE: Oh.

DR. MUSTARD: I guess, counsel, sometime...and I don't want to get into it now...but I would like to have further discussion of what you say the term short term includes in terms of the work population.

I don't think I should get into it now, but I think we should come back to it.

MR. LASKIN: It might be easiest to perhaps...easier for the witness...if he deals with his own remarks on the morbidity study and then we can come back and ask questions. That probably will facilitate everyone.

THE WITNESS: Okay.

So we've got a group of employees, all of whom were employed, some of them from one day or two days, up through thirty years. A group of them who I have chosen to call short-term, for lack of a better description, have disappeared from view. I just don't know what has happened to them.

Some of these former workers might have developed compensable chest disease, but not have received compensation from the Board. I was thus faced with the choice of introducing one of two biases - I could include all of the exposed workers in the analysis, and this would be anyone who has ever worked in the plant in asbestos exposure, and by doing so risk underestimating the effect of asbestos exposure since some of these people have disappeared and I don't know what has happened to them.

On the other hand, I could exclude these shorter-term people from my analysis and risk overestimating the effect of asbestos exposure in the lower exposure categories, which is where these people would appear by virtue of their shorter-term employment.

I selected the latter approach, which was to exclude the shorter-term employees, and arbitrarily decide to exclude from the main analysis all employees with less than fifteen years of employment. This introduces problems of bias and I've discussed this in the paper which has been submitted to you.

From a public health and compensation point of view, however, it was important to form some estimate of the numbers and exposure patterns of these shorter-term workers who might have developed unrecognized compensable disease.

THE WITNESS: (cont'd.) It so happens that the turnover patterns of this factory were such that most workers were employed either for very short or for very long periods of time.

5 By way of illustration, there were only forty production workers hired before 1960, who were employed for the shorter period of less than fifteen years who were employed between eight and fourteen years. I was able to trace many production and maintenance workers in this eight-to-fourteen year category.

10 Mr. Robert Stewart, of the Energy and Chemical Workers Union, kindly provided me with an introductory letter, and I wrote to the men in this shorter-exposure-term group, from eight to fourteen years, inviting them to have an examination during the next plant visit by the chest service, or to visit an x-ray clinic in their communities and to have the film sent to me for examination by Dr. Vingilis.

15 Nineteen of these men accepted the invitation, and following their examination I suggested to two of them that they file claims with the Compensation Board because in my opinion there was some evidence that they may have developed compensable disease.

20 None of the other men that we looked at had developed signs of asbestosis.

The selection criteria I thus used to define the study group, fifteen years of exposure...fifteen years of employment, rather...have produced a group of men at high risk for disease, and the prevalence of compensation is very high among them.

25 By July of 1980, thirty-nine percent of the hundred and fifty-seven production workers, and twenty percent of the forty-four maintenance workers had received compensation awards for asbestosis. These certified men have had mortality rates about five times greater than those from the general Ontario population, and their cancer rates have been increased about
30 ninefold, based upon official death certificate codings.

MR. LASKIN: Q. That's table one?

THE WITNESS: A. That's table one, yes.

5 The probability of certification was found to be related to the length of time the workers were employed in dust exposure areas. However, because of the lack of exposure response data in the world literature, I decided to attempt to go beyond the use of simply duration as a surrogate for exposure, and to attempt to use the employment histories and air sampling data to quantify the exposures.

10 The amount and quality of sampling data available was limited, but in view of the void in our knowledge we felt that an attempt at a quantitative analysis was justified.

I thus used the company's personal membrane filter sampling data from the years 1969 and 1970 as the basis for an exposure model, and calculated cumulative exposures for each of the production workers.

As I have already mentioned, I didn't think that this was possible for the maintenance workers and I have excluded them from the quantitative analysis.

20 The exposure model that I used, and the uncertainties associated with it, are discussed in the report which has been submitted to you.

Workers were placed into exposure categories based upon the exposure accumulated during their first eighteen years of exposure. This again was an arbitrary decision which was based on the observation from figure one that only two awards for asbestosis were granted prior to this time interval, so I asked the prospective question: Given an accumulation of asbestos exposure up to this point in time, what is the subsequent risk of developing compensable disease?

30 The next slide, which is figure two in this paper, illustrates the cumulative probabilities for workers in each of the exposure categories, who have been certified, as a function

THE WITNESS: (cont'd.) of time since first exposure.

5 It can be seen from this graph, or from this table, that the probability of certification depends both upon time and upon the previous intensity of exposure.

If there are any questions about this figure at this point, I would be pleased to address them.

10 MR. LASKIN: I think we will all have a number of questions. Why don't you just summarize the remainder of your remarks on the morbidity study, and then we'll come back, because I think all of us will have a lot of questions on these matters.

THE WITNESS: Okay.

15 So this figure two shows what has happened to men as a function of time and exposure in the study group. The more important...or an important question is the relationship between the exposures and the eventual probability of certification.

20 I investigated this by calculating the cumulative probability of certification by the maximum possible followup interval, which has been thirty-two years. The results is illustrated in figure three, and it can be seen that the risk of asbestosis increased with increasing exposure, and for this particular factory cohort the relationship between these two variables - that is, exposure and probability of certification - is S-shaped, or sigmoidal in form.

25 I have drawn a curve through these points and this curve is the mathematical function which is called the cumulative logarithmic normal, and it's a remarkably good fit to this data.

30 This particular mathematical function is frequently observed in studies of chemical and drug toxicity, and it is possible for asbestos toxicity as well.

From this graph you can see that there is a low,

THE WITNESS: (cont'd.) but finite, probability of response at low exposure levels, and that saturation or virtually complete response occurs at high exposure levels.

5 The response at low levels suggests that for all practical purposes there is probably an effective threshold below which clinical asbestosis will be unlikely to occur, although there could be the occasional very susceptible individual who could develop problems at these low levels.

10 Since asbestosis is a progressive condition, however, great caution must be exercised in extrapolating these results beyond the timeframe of this study, which is thirty-two years. Extrapolation of early results to working lifetime, as some have done, is a very uncertain business.

15 That basically, are my remarks about the morbidity study.

MR. LASKIN: Thank you, Dr. Finkelstein.

I wonder, Mr. Chairman, if we might just take our morning break for ten minutes or so?

DR. DUPRE: Fine.

20 THE INQUIRY RECESSED

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THE INQUIRY RESUMED

MR. LASKIN: All set, Mr. Chairman?

DR. DUPRE: Please proceed, counsel.

MR. LASKIN: Thank you.

25 MR. LASKIN: Q. Dr. Finkelstein, can we now turn to various aspects of your report at tab four, and perhaps explore them in a little more detail?

30 Before we get to your exposure-response analysis, could I just ask you one or two questions about your results at the top of page seven at tab four, where you deal with the question of the cumulative probability of certification?

THE WITNESS: A. Yes.

5 Q. All right. You say that the probability of risk was sixty-eight percent for men exposed fifteen years or more, and do I have that correctly that what you have done is taken those production workers who were exposed to asbestos for more than fifteen years, which in accordance with your figures is eighty percent of the hundred and fifty-seven, and of that group, eighty percent of a hundred and fifty-seven, sixty-eight percent of those, during your time study, in fact became
10 eligible for compensation?

A. Not exactly. There is a difference between... this is a technical question...there is a difference between what's called prevalence, which is the percentage of any particular outcome in a group of people, and cumulative probability.
15

Q. That's what I was getting to. Can you help us, because you use the term cumulative probability in your studies, can you elaborate on what that distinction is?
15

A. Okay. The problem is that some of these men died and never lived to reach thirty-two years. If I included someone who was run over by a bus twenty years after first exposure in calculating the risk of developing asbestosis by thirty-two years, I would underestimate the risk.
20

If there were a hundred guys in the group and ninety-nine of them were in a plane crash, and the one survivor developed asbestosis, then the prevalence is one percent, but the risk, in this naive example, is a hundred percent.
25

So in calculating the cumulative probability, I have taken account of two things: The first is men who have not been followed long enough to reach the thirty-two years. A guy starting work say in 1955, who would have had a maximum followup of twenty-five years, so he has only been included in the risk assessment as long as, you know, he has so far been eligible
30 for following. So that's one thing.

THE WITNESS: (cont'd.) The other thing is deaths. A number of these men have died and so had they lived longer, they might have developed compensable asbestosis, but since they were dead they were, you know, just incapable of being compensated for asbestosis. So in calculating the cumulative probability I have used the technique whereby I take into account so-called withdrawals, which is the technical term for people who haven't yet been observed long enough to reach the maximum interval, or who have died prior to reaching the maximum interval.

So the cumulative probability is the probability which you would accumulate if you survived long enough, in this case thirty-two years. What is your chance if you don't die from other causes or, you know, if we can follow you long enough, what is your chance of being certified by thirty-two years. And that's what the cumulative probability is.

MR. LASKIN: Q. If I'm an employee in your cohort, having a minimum employment of fifteen years and to use this also fifteen years exposure to asbestosis, and I survive thirty-two years from my first exposure, I have a sixty-eight percent likelihood of being compensable for asbestosis within that period?

THE WITNESS: A. Accurately put, you would have had. You are asking me to predict into the future, which is another question, but this has been the experience of this group of people.

MR. LASKIN: Did you want to follow up on that? Okay.

MR. LASKIN: Q. Now, can we turn to your exposure analysis for a moment, and can you, because your exposure calculations are, I take it, the same, you have done the same fiber calculations for your morbidity study as you have for your mortality study?

THE WITNESS: A. Yes, that's right.

Q. Even though I note you've got different fiber classifications as between tab four and tab five.

5 A. Only in the sense that in the mortality paper I just divided the group into three equal pieces. Here I've split them up on the basis of the actual fiber levels.

However,...okay, we'll come to it.

10 Q. Can you...can we explore, just for a moment, in a little more detail, how you made these calculations, and I take it from your paper that you started with a figure which you knew, which was a personal sample membrane filter measurements in 1969 and 1970, and then you worked backward in time on the basis of the three assumptions that you set out in your paper?

A. Yes, that's right.

15 Q. All right. Can we just go to page ten of tab four where there is some detailed discussion of what you have done?

20 Dealing first of all with assumption A, do I understand that assumption to mean that regardless of what the level of dust may have been for any particular job, the relationship between the jobs, in terms of their levels, never changed?

A. Yes. If job A was twice as dusty as job B in 1970, I assumed the same was true in 1953.

Q. Or in 1948?

A. Or whatever, yes.

25 Q. All right. Was there data that supported that assumption?

30 A. The impinger area sampling data done by the company and the ministry supported this particular assumption. I described it as weakly...with an 'a' rather than an 'e'... because there are very large...there is a coefficient of variation, a technical term. There is a lot of scattering of data, okay?

A. (cont'd.) It's just not very accurate and not very precise.

5 Within the limitations of this inaccuracy and imprecision, it would appear from the impinger area data, which I must emphasize is different from the personal membrane data, that this assumption is not unreasonable.

Q. Can you tell us in a little more detail just how much data you have? I mean, how many samples were taken and were they correlated with job areas and how often, and so on?

10 A. Okay. The Ministry of Labour first went in to make measurements in the plant in 1949, at which point they did roughly six to ten measurements in various areas in the plant.

Q. Over what time period?

15 A. This was done in one morning. The measurements were twelve to fifteen minutes each. This is the kind of data that we are working with in this field.

Q. Was that it for the year? That was it for 1949?

20 A. That was it for 1949, until 1954, when an insurance company, Travelers Insurance, who were, I guess, hired by the company, went in and did a survey in 1954. The company has provided me with their results. This is proprietary information of the company. If they are agreeable to releasing this, I can let people look at the kind of numbers we are dealing with.

25 MR. PATTERSON: I have not seen the data, so I have got no idea what it is. I don't think I'm in a position to...

THE WITNESS: Okay, in any event...

MR. LASKIN: We can hold, we can reserve on that and consider that document later.

30 THE WITNESS: I don't think it's crucial, but the next measurement was made in 1954 by insurance company hygienists for the company. Their...

MR. LASKIN: Q. Just before you get to that, were

Q. (cont'd.) any measurements made between 1949 and 1954?

THE WITNESS: A. No.

5 Q. Okay. So we have, up to 1954, just the one day of Ministry of Labour measurements, and then we skip to 1954?

A. That's right.

There were perhaps two dozen samples taken in June of 1954, by insurance company hygienists, at various areas in the pipe plant and elsewhere.

10 In 1955, the Ministry returned and made measurements in the flex board shop and in the pipe shop. I can't off the top of my head tell you how many samples were done. There were perhaps two dozen.

Q. Again on one day, probably?

15 A. On one or two days. The actual sampling time at each station was less than half an hour, I think, about fifteen minutes.

Mr. Nelson, here, could probably give us more information.

20 But 1955, in 1957, the insurance people returned, made measurements in the same general area that they had measured in 1954. Again, there were roughly two dozen measurements, again done by impinger, presumably each sample was fifteen minutes or half an hour.

25 Q. Were there any comparable government measurements at the same time as the insurance company was in making its measurements?

30 A. The government was in in 1955, the insurance company was there in 1954 and 1957. So, you know, this is what we are dealing with. One day in one year, five years later you come back and you make another measurement, a couple of years later you come back for a couple of days and you measure again.

Q. Even given the difference in years, because

5 Q. (cont'd.) you have made certain assumptions that the measurements were roughly uniform over, say, the period 1955 to 1962, how in fact did the government measurements compare to the insurance company measurements?

10 A. The government and the insurance company measurements were similar. The government returned in 1961 and did another series of measurements. The company, in 1961, began doing their own inhouse hygiene surveys. They were done some... well, occasionally several times a year, but at least annually, and they set up a number of fixed areas in the plant where they would do their impinger area surveys, and they would come back to the same place for the next survey.

15 So these were...so we have company data, impinger data, from 1961 up until the plant shut down, in essence. Beginning in the autumn of 1969, the company began doing personal membrane filter sampling...

20 DR. DUPRE: Excuse me, Dr. Finkelstein. Just before we get to 1969, can I just get the following straight? Can I take it that all of the measurements pre-1969, whether governmental, insurance or company, are by the midget impinger?

25 THE WITNESS: Yes, that's right.

DR. DUPRE: Therefore, all these measurements are measurements of dust and...

THE WITNESS: Total dust.

DR. DUPRE: ...in million particles per cubic foot?

30 THE WITNESS: That's right - asbestos, cement and silica in unknown proportions.

In 1969, October, November of 1969, the company began doing their membrane filter sampling data. This was subsequently done on a quarterly basis, four times a year. They would put pumps on various employees and sample them.

35 Off the top of my head, I can't tell you what the sample duration was. It was several hours to...I think less

THE WITNESS: (cont'd.) than a work shift.

Again, this was done on a given day, four times a year.

5 In my analysis of the data there was an indication that the dust counts were seasonal, and that during the winter when all the doors were closed and they recycled air, the fiber counts tended to be higher than during the summer when the doors were open.

10 The variation appeared not to be...well, it was about ten percent or so. I decided that, given the enormous uncertainties otherwise in the data, that there was little point in taking any account of this seasonal variation. I presume that the seasonal variation must have occurred at earlier times as well.

15 So that's essentially the data I worked with.

In the summer or autumn of 1970, the company made major ventilation changes in the pipe plant. The flex board shop was shut down during the summer of 1970, and so there were no subsequent measurements made there.

20 Following 1970, I used the actual measurements of dust. In other words, there are no extrapolations or interpolations there.

MR. LASKIN: Q. Of fibers?

THE WITNESS: A. Of fibers.

25 So basically I took company data from 1969/1970, which was a measure of fibers greater than five microns in length. I looked at the trends in the impinger data from all three sources - company, insurance company and government - to form some crude estimate of how conditions might have changed in time, and I constructed my exposure model based upon that.

30 I have a slide, actually, or a transparency, which shows how all the other witnesses have handled this problem as well, so I think that...

Q. I'm going to come to that.

A. ...what I have done cannot be looked at in isolation. All these studies have to be pooled and have to be compared with what other people have done.

Q. We'll come to that slide, but just so that I understand, what you then did was you assumed that the measurements in 1969/1970 were the same measurements as prevailed from 1963 forward?

A. From actually the winter of 1961 forward. Ventilation changes were made in 1961 as well.

The very valid point that the company has made to me that there are two components to ventilation - there's gross ventilation changes and there's local machine-related and job-related hygiene improvements. Given that this is all area sampling, although the company wheeled their impingers around on little carts and tried to get them close to the employees' noses, so they were close to the workers, but they were different from personal sampling data, it's absolutely impossible for me or for anyone else to assess how...what each worker was actually exposed to and how to take into account, you know, the little tinkering that was done with the exhaust on any particular machine.

Q. All right.

A. That's a universal problem.

Q. So between 19...the winter of 1961 and 1969/1970, the assumption was that the measurements in the various job categories were the same?

A. Yes.

Q. How many job categories are we talking about, roughly speaking?

A. A couple of dozen.

Q. A couple of dozen? Okay.

Then between 1955 and 1961, you assumed that the measurements were thirty percent higher and does the thirty percent

Q. (cont'd.) figure turn on the impinger data, as weak as it is, or on the fact of ventilation, or what?

5 A. Yes. This was based on a calculation I did comparing the pre-1961...I guess maybe it was 1962, I think, actually, the pre-1962 ventilation change company impinger measurements with the post-1962 company impinger measurements. The mean ratio was thirty percent higher prior to the ventilation changes.

10 By chance and coincidence, the ratio of the ventilation capacity before and after was also thirty percent. I think that's entirely gratuitous.

15 So this is just a guess, analogous to the work that was done at Rochdale where they guessed it was twenty-five percent higher during the mid-forties, and fifty percent higher before. There is some indication from the impinger data that thirty percent might be a reasonable number, but it's pulled out of thin air.

20 Q. I was going to ask you about Rochdale, because you referred to the way in which Berry calculated his previous dust levels, and as I understand it he had at one stage some side-by-side fiber count measurements and impinger measurements.

A. Certainly not before 1970.

Q. No.

A. They never used impingers at Rochdale.

They used..

25 Q. Thermal precipitators.

A. Thermal precipitators, yes. They used...they made measurements in 1952 and in 1960. I guess in 1961, they had some side-by-side...

30 Q. But as I understand it, what they did was then take a ratio of the thermal precipitator counts at various points in time and applied the ratio as against the fiber measurements that they knew at roughly one point in time?

A. Yes. They compared the 1952 with the 1960.

5 Q. I guess my question to you was whether you considered taking the 1968 impinger data that you have and working out some ratio in the same manner as Berry, looking back?

10 A. I didn't think that the quality of the data would improve. You know, I haven't seen the British factory data. Based on what I've seen of the data from this plant, I am rather skeptical about the accuracy with which, you know, the British hygienists were able to make their ratios. I think we are
15 talking about one point seven in one job, and three point two for another job. One explanation is that dust conditions were better. The other explanation is that this was entirely chance. I think they are both equally plausible.

Q. Fair enough. Okay.

15 Just one or two other questions on this. How did you arrive at the factor of two for the period 1948 to 1954, and I guess I ask that question because I note in some of the text you refer to how dusty the conditions were in the period...

A. Yes.

20 Q. ...and I note for example you say in the first full paragraph on page ten, "The handful of measurements from 1949 are generally several times larger in value than the later measurements".

A. These are the ones that were done in one morning.

25 Q. Okay.

A. A factor of two, again, is arbitrary, in a sense. I had discussion at the plant with Mr. Shelvington, who was an industrial engineer, who designed the 1970 ventilation change. He had been present and working in the plant in the early fifties. He drew for me a little graph which was his
30 impression of how dust conditions had changed in time, and he said things were pretty bad back then. They were twice as bad.

5 A. (cont'd.) Dr. Chase, the company statistician, arranged a telephone conversation for me with a Mr. Mulholland, who also, I believe, is an industrial engineer who was present in 1947 when the plant was being constructed, who worked at West Hill until about 1969, when he was transferred to head office in Denver.

I asked him, you know, I'm thinking about using a factor of two, does this seem reasonable to you? He said, yes.

10 So arbitrarily it's based on the universal opinion of people that things were pretty bad back then, certainly worse than in 1969, probably worse than 1961, and so I chose a factor of two.

Q. I don't at all mean to be critical in asking the question. I think you have been very fair in pointing out the uncertainties.

15 A. I think this is a crucial kind of thing to address. Every occupational epidemiology study makes use of data like this. Of relevance to Ontario are conditions in uranium mines. The current risk estimates for uranium miners are based on measurements in American mines where perhaps once a year they go in and grab a sample in one mine, and ten years later they go into another mine and grab another sample, and this is the kind of data that we are working with in these studies.

20 Q. Dr. Uffen has a question, but I take it before your testimony is over you are going to give us the benefit of your assessment of some of the other studies on this issue?

A. Well, why don't we do that right now.

DR. UFFEN: I have a specific question about the factor two, which can be dealt with quickly.

30 Did you have an opportunity to assess the sensitivity of your results to a factor of two? Suppose you had made it four, would it have made any difference? Because

DR. UFFEN: (cont'd.) it applies to the people that were there thirty-five years.

5 THE WITNESS: Certainly the risk assessments are crucially dependent on the exposure estimates. One way I looked at this in the mortality analysis was to assume I had underestimated them by a factor of ten, and so you've got, you know, a range from the values I selected to results you would get if I had guessed wrong by a factor of ten.

10 MR. LASKIN: May I suggest that we come to that when we go to the mortality study? If we are thinking about the same...

THE WITNESS: No, we are not. I have a transparency there which looks at how each of your witnesses has calculated their exposures.

15 MR. LASKIN: Q. Good, that's okay.

THE WITNESS: A. Okay. I've given you a paper, hardcopy handouts of all this stuff. Somewhere in there, there is a transparency as well.

MR. LASKIN: Why don't you give us all just a couple of minutes to look at this, and then we'll be up with you.

20 THE WITNESS: Sure.

DR. DUPRE: Counsel, will this acquire the title of tab six, or what is your pleasure?

MR. LASKIN: Sure, why not. It will be tab six of exhibit thirty-six.

25 EXHIBIT #36, TAB 6: The abovementioned document was then produced and marked.

DR. DUPRE: Could I maybe, while we are on that, set one other thing straight for the record? Am I correct in saying that tab four and five were originally exhibits five and six?

30 MR. LASKIN: That is correct.

DR. DUPRE: Thank you.

MR. LASKIN: In other words, just so that we are clear, those are the two documents that got submitted the day after Dr. Enterline's testimony.

MR. LASKIN: Q. All right. Tell us what we've got on tab six here, Dr. Finkelstein, and what you've done.

THE WITNESS: A. Okay. Tab six contains a list of the four studies, four factory studies in the world, with the exception of John Dement's, which would be number five, where there is any attempt at quantitative risk assessment or dose-response assessment. That's Enterline's Johns-Manville retirees studies, Hans Weills' Louisiana studies, Geoff Berry's asbestosis study which is from the same factory that Peto used, and I have omitted Dement's study.

Q. Are these, when you refer to Dr. Weill's study, for example, you are talking about his morbidity study or his mortality study?

A. Morbidity, mortality, it's all...he and I have used the same...for John Doe, who appears in both studies, he has got his exposure.

John Dement, actually, we have obtained a copy of his dissertation and I think the Commission has a copy as well. I have not yet had a chance to digest it, but I might say from what I've looked at, it seems to be an extremely thoughtful and intelligent approach to the very problem of exposure assignments, and is probably the best thing I've ever seen in print.

Q. On this issue?

A. On this issue, yes, of exposure assignments. I have some quarrels with what he has done with it, but I think from his approach to the data, given the caveat that I have just skimmed it and when I look at it more carefully I might change my mind, but on a skimming it looks pretty good.

5 A. (cont'd.) So anyway, on this tab, these are the four studies. This is everything that is known in the world, with the exception of all the mining data, about relationships between asbestos exposure and health effects.

Q. In other words, these are...apart from Dr. Dement and apart from the mining studies such as Dr. McDonald's... these are the four studies that really provide a quantitative risk assessment or a quantitative dose-response analysis, correct?

10 A. Right. There are a couple of other mining studies which tie in, but I have covered myself by explicitly saying these are the factory cohort studies.

So what I've done here is, I have quoted from these gentlemen's papers what they have to say about their exposure estimates.

15 The first two, Enterline and Weill, both study Johns-Manville factories. Dr. Weill told us in testimony here that he never saw the exposure data himself. This data was supplied to him already predigested by the company.

Dr. Enterline told me the same thing himself.

20 Of interest is the fact that the Toronto plant was included in Dr. Enterline's study, so the company hygienists have done something with this data in order to provide Dr. Enterline with their guess as to what the exposures have been. You know, there are quotes from his paper.

25 Enterline, for example, has said, "the cumulative exposures were estimated based upon midget impinger counts. It was not always possible to be very precise, to very precisely estimate the dust level for each job, particularly for past periods, and often dust levels could only be roughly classified. Jobs were placed in one of six classes, and for calculating total exposure, midpoints of the range were used."

30 He describes the classes that he used.

It's of interest that the insurance company data

A. (cont'd.) that we've got do not fall any higher than his second exposure range, which is five to ten million particles per cubic foot.

5 So that's Enterline's data. Hans Weill, in describing his Johns-Manville plant, says that the company provided him an exposure coding for each job, based on midget impinger sampling data. That is all he has to say about it. Presumably it was calculated in exactly the same way as Enterline's.

10 Berry describes what they did, which is again what we have just discussed - taking the ratios, and Peto has used the same data. As you know, company hygienists at Rochdale are now recalculating their data. One sentence which Peto says which is relevant, I guess, which says, "The process inevitably involves a good deal of speculative adjustment", so again, they are guessing.

15 Q. Did you, yourself, by the way, see the raw data?

A. Absolutely. I did all the calculations myself.

20 Q. From the raw data that you obtained? Okay.

I know we are going to come to this, but do your comments here in part bear on your judgement as to whether any or all of these studies are usable from the point of view of doing a quantitative risk assessment?

25 A. They certainly have a bearing in the sense of the accuracy and reliability that you can place on the risk estimates. I noted when Dr. Enterline was here, he had a slide showing you SMR's calculated from these various studies, which put it to four significant digits.

30 I think that has to be placed in the context of this kind of exposure data. You know, if he says I found an SMR of a hundred and forty-seven point three six five, and Hans Weill was a hundred and ninety-two point four seven three, based

A. (cont'd.) on the fact that everyone is guessing about all this stuff, you know, it's a little bit questionable.

5 Q. But I have in mind a comment that appears not in tab four or five, but in fact in tab two, at page 88 of tab two. I may be taking you away from what you are doing, and you tell me, but you make the statement in this document which you prepared for the Ministry, where you say there is only one study available in the literature which gives a useable risk estimate, and this is the study of Peto, based on the mortality experience of the BOHS factory cohort, and I take it that's the Rochdale cohort?

A. Yes.

10 Q. Now, has your...from the time you made that statement to the present, has your judgement changed in any way about whether there are any other useable studies?

15 A. I will reserve comment on that. During the month of August I will be rewriting this criteria document for the Ministry with respect to the public meeting for the asbestos regulations, to be held in mid-September. I am sure the Ministry will make available to you and to the public what I write, and I prefer to wait until that time before commenting further on this question.

20 Q. Fair enough, but when you made this statement, can I ask you whether you had in mind, as I recall, the four studies that the Simpson Report used in assessing its quantitative risk?

25 A. Peto's was the only study which talked about fibers. All the others are based on total dust.

30 The Ministry was proposing to regulate asbestos based not on total dust, but on fibers. It therefore seemed appropriate to look at whatever information was available about fibers, and this is it. This is the only study. It remains the only study, apart from Dement's and the work I've just done.

5 Q. All right. Perhaps we can explore that. I know you have another transparency which deals with that in more detail, and perhaps we can deal with it when we get to the mortality study.

Okay.

Is there anything further you wanted to say about tab six, the document we have just introduced?

10 A. Only in self-defence. I think what I've done is very uncertain. I don't think that it can be looked at in isolation. It has to be looked at in comparison with what other people have done. Certainly by the standards of the physics or chemistry laboratory, all this would be abominable.

By standards of epidemiology, this is the kind of data we work with.

15 DR. UFFEN: I have a question which I think maybe later on...it's just that the translating from the way Berry presented his data to the way you presented yours in your final mortality, the probability of asbestosis here...

MR. LASKIN: We are coming to that.

20 DR. UFFEN: You are coming to that?

MR. LASKIN: We are coming to that.

MR. LASKIN: Q. Can we...and we are going to get to that...can we just take your tables at the back, and I want to make sure that we understand...I'm now back on your morbidity study, before we get to it...but can we just go through the tables that we haven't gone through, and make sure we all understand what they are about? Can we go to table two, first of all, which is, I take it, an incidence study, a prevalence study?

25 THE WITNESS: A. Yes. Well, it's not a study, it's a calculation.

Q. Is incidence the same as prevalence?

30 A. No. Incidence is the rate at which things happen. So many cases per year. Prevalence is the numbers that

A. (cont'd.) are present, which is when you add up the incidence, you end up with the prevalence.

5 Q. Okay. If we take your first dust category, zero to forty-nine fiber years per cubic centimeter, I take it there were twenty-four of your production workers who, in eighteen years, were within that category?

A. Yes.

10 Q. Then I take it after the eighteenth year, each of these men then started contributing person years at risk to these various...

A. Yes.

Q. ...year categories?

A. Yes.

15 Q. All right. Within this category, zero to forty-nine fiber years per c.c., there was only one of the twenty-four persons who became certified or compensable for asbestosis, and that happened in the twenty-fifth to twenty-sixth year?

A. Yes.

20 Q. Then you did a man-years calculation on each year category, is that correct? All of which would, of course, be zero except for the category twenty-five to twenty-six, where the one person fit in?

25 A. Well, I wouldn't call it a man-years calculation. I would call it a calculation of incidence, which is essentially the ratio of the number of cases appearing in any interval to the person-years of risk.

Q. All right.

A. So if you look at the total column, there's one case, a hundred and ninety-six person years at risk, so the incidence is half a case per hundred years, I guess.

30 Q. Does that, point five, what does that signify? What does the point five mean? Does that mean that if you have

Q. (cont'd.) been exposed to between zero and forty-nine...

5 A. Actually, the only reason I did this was to compare it with what the British have done. I think the interpretable data is the subsequent data on the probabilities.

Q. What's the significance of this table?

10 A. It depends on what you mean by significance. The significance is that it gives me something to compare the only other data in the literature with. Peto likes to talk about incidence. When he comes tomorrow, he will probably tell you about incidence. Berry has described incidence in his, so doing this calculation it gave me some numbers to compare with other people.

15 It's just a midpoint...or not a midpoint, it's a step along the way to what I considered to be the relevant results, which are the probabilities. It was just an intermediate sort of calculation for comparison purposes.

20 Q. Can I ask you, because I added up the men in table two and I got a different figure than the one fifty-seven, I got one fifty-two.

25 A. Yes, that's right. It's because I omitted from this table the men in the greater-than-two-hundred-and-fifty category, who are five in number. If you add up the men on figure two, you'll find there are an extra five men in the group there, which comes to one fifty-seven.

30 Q. Can I ask you this, do you use the calculations in table two in any of your subsequent probability calculations?

A. No, only in comparison with Geoff Berry's work.

Q. Which is in your paper?

A. Yes.

Q. Okay.

35 DR. UFFEN: I think this answers a question I have. I'll just tie it down. On page three of this paper we discuss in

5 DR. UFFEN: (cont'd.) tab four, you refer to, in the second paragraph, Berry's results. For example, at one hundred fiber years per c.c., he had a prevalence of thirteen percent.

THE WITNESS: Yes. Prevalence here means that if there were a hundred men, thirteen of them were certified.

DR. UFFEN: If I were to try to find a comparable figure in your table two, would there be one?

10 THE WITNESS: You have to integrate the incidence.

DR. UFFEN: Okay.

THE WITNESS Incidence is essentially DN/BT, and you integrate that and you end up with prevalence.

MR. LASKIN: Q. Do you know offhand if you did that integration what figure you would come up with?

15 THE WITNESS: A. Somewhere in here I compare, I make some assumptions, I talk about prevalence. I can't give you the page number, but it's somewhere.

Q. I think that's pages sixteen to seventeen.

A. Page nineteen, paragraph two.

20 Q. Page nineteen.

A. So I compared prevalence with cumulative probability, which is not exactly the same thing, but it's sort of comparable in this context.

Q. That's what I want to come to is that whole issue.

25 A. Okay.

DR. UFFEN: I hope I'm not getting ahead of you, but when I do a little numerical integration in my head, partway down, are your results going to be very much larger than Berry's?

30 THE WITNESS: In the timeframe of Berry's study, they appear about the same. I've got an extra ten to fifteen years of followup, so...and if you look at figure one, or whatever

THE WITNESS: (cont'd.) it is, that shows the time course at certification, you will find that when you integrate beyond his time, that's when you start finding cases.

5 DR. UFFEN: Thank you very much.

MR. LASKIN: Q. Can we go then to the next figure that we haven't discussed, and that's figure two? I think it would...

10 DR. DUPRE: May I ask a question before you go to figure two?

MR. LASKIN: Sure.

DR. DUPRE: We've been looking at table two, which is on the incidence of certified asbestosis, and I just want to clear up in my mind whether or not there is any relationship between what I see in table two and the discussion on page seventeen of the text, where about, oh, nine lines, nine or ten lines down?

THE WITNESS: Yes, this is where I'm comparing this table...

DR. DUPRE: You talked about calculated incidence.

20 THE WITNESS: ...yes, with the table by Berry and Lewinsohn.

DR. DUPRE: Yes. Can you just help me a little bit with the following: You have, in your three categories, zero point seven percent, one point six and two point four.

25 Berry's percentages are zero point four, one point zero and two point zero.

Now, those figures I have just quoted you have at the bottom of page sixteen, by the way, but I just wrote them down there.

30 Now, as a layman, here I am eyeballing zero point seven versus zero point four, one point six versus one point zero, two point four versus two point zero. Can you tell me what I should make of those differences, if anything?

DR. DUPRE: (cont'd.) Your percentages are consistently high.

5 THE WITNESS: I don't think you should make anything about the differences. The conclusion I have drawn is that my results are not wildly out of line with his, or vice versa.

10 I think anything within a factor of two or three in this kind of work is excellent agreement. You know, my intention in making the comparison was, am I way out of base or way off line, and looking at the only other piece of data in the world, it would appear that, you know, the experience in Ontario was somewhat similar to the experience in England.

15 MR. LASKIN: Q. To get your figures...just so that we are all clear, at least I'm clear...did you take, to compare your figures with those of Berry, did you take table two and cut it off after the twenty-third year?

THE WITNESS: A. Yes. Which again...

20 Q. And recalculated your incidence rates based on a time period up to twenty-three years rather than thirty-three years?

A. Yes.

MR. LASKIN: Are we all clear on that?

MR. LASKIN: Q. Then can we now address figure two, and can you go slowly and tell us what you have done here and how you have arrived at it?

25 THE WITNESS: A. Okay. Well, there are six groups of men, A through F, who fall into the exposure categories that are listed there, and the number of men in each of these exposure categories is indicated in parenthesis.

30 What I have done...everytime...well, I guess the thing to do is select a group, group A, which is the easiest one, down at the bottom, just the pure dots. Everytime a man is certified, there is a little jag in the curve, okay?

5 A. (cont'd.) So this gives you the time course at which certification had occurred, and again, the cumulative probability...well, so if you look at any point along here, let's take twenty-five years, and look vertically up from twenty-five and see what the probability has been that if a man were alive at twenty-five years after initial exposure, what would his probability be of having been certified by that point in time.

10 So that's essentially what these various lines show you.

Q. What would it be? Would it be the lower part of the jag, or the upper?

A. Well, the fact that this one jags at twenty-five, if you move over a millimeter or two, it doesn't jag much there.

15 Ultimately, I use this again as an intermediate step to the final result, which is on figure three, which is the thirty-two year cumulative probability, which is what you get when you draw your line up at thirty-two.

20 Q. Could I, sitting at my desk with only the information that you've got in tab four, could I make the calculation that appears in figure two, or indeed figure three?

A. No, you couldn't. You would have to know who died and when, and who was certified and when.

25 DR. DUPRE: Could I ask this, Dr. Finkelstein, your bottom dotted line that jags just after the number twenty-five, does that jag right after the number twenty-five because the top line in table two showed one individual who became certified between his twenty-fifth and twenty-sixth years?

THE WITNESS: Yes, this is him. Since there were only twenty-four men in the group, and since...well, yeah...so it increases the probability from whatever it had been at that point to almost...to about ten percent.

30 MR. LASKIN: Q. The other jag didn't make it into the table two because it occurred before the nineteenth year?

THE WITNESS: A. Yes.

Q. But it's reflected in figure three.

DR. DUPRE: Does the initial jag then in table two,
5 is the before-nineteenth year case?

THE WITNESS: Yes.

DR. DUPRE: Okay, thank you.

MR. LASKIN: Q. Okay. Do you use figure two
to get the figure three?

THE WITNESS: A. Yes, by drawing a line at
10 thirty-two years, I read off the cumulative probability for each
of these five exposure categories, and I plot the thirty-two
year value on the graph on figure three.

Q. Just let me make sure I've got that. If I
go back to figure two, and draw a vertical line at thirty-two
15 years since first exposure, I will then get a series of six
points?

A. Six points. I cheated here by leaving off
the last group, which only contained five men.

Q. That's the excess two hundred and fifty?

A. Yes.

Q. So I will then get five points?

A. Yes.

Q. And I take those five points and I then plot
them, as you have done the little extras on figure three, and
you then fitted a curve. Is that it?

A. That's exactly it.

DR. MUSTARD: Can I ask a question? If you put
25 in the sixth point, I guess it would have been...

THE WITNESS: The sixth point...four out of the
five men were certified, so that's eighty percent. As it turns...

DR. MUSTARD: You would have just kept your plateau.

THE WITNESS: As it turns out, the fifth man
30 died ten years after he retired. I received an x-ray from Orillia,

THE WITNESS: (cont'd.) wherever he died, which Dr. Vingilis looked at, and he had asbestosis on his x-ray.

So he had asbestosis, but he wasn't certified. So in the two hundred...greater than two hundred and fifty, if you took him into account, it would be a hundred percent.

But that's cheating, since he didn't meet my fickle criteria.

MR. LASKIN: Q. Okay, now I want to take figure three, and I would like you to tell us how you use figure three to arrive at what fiber level will give you, what cumulative exposure will give you a probability of developing asbestosis, of one percent, which is the, in the text, appears in the second full paragraph on page nineteen.

THE WITNESS: A. Okay. The line drawn there is the shape of a mathematical function called the cumulative logarithmic normal. There is a mathematical equation which describes what the height of this line will be at any place along the axis.

I used the formula for this equation, and I plugged in the probability of one percent and came out on the Y axis with the number ten.

Okay, so it's derived from the mathematical equation which describes this curve.

Q. If you look at figure three, I take it the one percent would be at the point zero one level on the vertical axis?

A. Yes.

Q. So if we had a slide, if we had a drawing big enough and we marked the point zero one mark on the vertical axis, and we drew a straight line to your curve and then drew a line down, we would get the fiber level?

A. Yes. This curve here is drawn by hand. It was drawn with felt tip pen, which makes a rather thick line.

5 A. (cont'd.) I wouldn't draw too many conclusions from what it actually looks like down at the low end. The numerical results come from the actual mathematical equations describing that curve.

10 DR. UFFEN: Would you have examined the sensitivity of the equation? Down in that very small exposure range, would several different parameters in the log normal curve equation fit just as well down in there? But perhaps not fit the rest of the curve?

THE WITNESS: Well, on probic graph paper, there is a straight line goes through these five points. So the slope, the standard deviation of the slope is very small.

15 I don't think that's the crucial factor. I think the crucial factor is that the exposures are uncertain. You know, any niceties in what this equation is telling you are overshadowed by the uncertainties in actually what the exposures were in these men.

20 DR. UFFEN: You may know that, and other people may know that, but the day may come when someone will use that curve who doesn't know it and start to attribute to it a degree of precision in that range that is unjustified?

That's what is on my mind.

THE WITNESS: That could and very well may happen. I refuse to accept responsibility for the misuse of my data.

25 DR. UFFEN: But my point in asking it is, that it is really an interpolation in a region where...

THE WITNESS: It's an extrapolation.

DR. UFFEN: ...an extrapolation towards the origin?

THE WITNESS: Absolutely.

DR. UFFEN: All right.

30 MR. LASKIN: Q. I'm sorry. I just want to, before I forget it, make one, make sure of one thing. This figure three

MR. LASKIN: Q. (cont'd.) only is a probability calculation in respect of persons exposed for thirty-three years since first exposure?

5 THE WITNESS: A. No. This takes into account people with shorter periods of exposure, people...you know, someone starting in 1960, for example, or 1959, will have the capability of having been followed for a maximum of twenty-one years. There are mathematical techniques which I have used to adjust for that.

10 Q. In this figure three?

A. And in the preceding graph, figure two.

Q. Okay.

15 A. I have withdrawn, as they say, people who died and people whose followup was less than the maximum. These are standard techniques used in analysis of drug experiments, cancer survival, all kinds of things. It's called the life table technique.

20 Q. But to assess...looking at table three, to determine a probability of certification for asbestosis, what criteria, what people can come within figure three? Is it only those people who have...

A. This...

Q. I'm sorry. You tell me.

A. This result is entirely specific to the hundred and seventy-two men that I studied.

25 Q. Hundred and fifty-two?

A. Hundred and fifty-two, or whatever. You extrapolate to other routes at your peril.

Q. A hundred and fifty-two men who appear on table two? Okay.

30 MR. LASKIN: I'm sorry, Dr. Mustard.

DR. MUSTARD: I would just like to rephrase Dr Uffen's point in a slightly different way to see if you would

DR. MUSTARD: (cont'd.) agree with this.

Dr. Sackett, when he addressed us at our phase one... not phase one, but our public meetings...I'll get that straight... gave a very clear account of the problems of trying to be too precise when you get down to the small values that you have at the bottom end of this curve, and I take it you would agree with that point?

THE WITNESS: Oh, sure.

DR. MUSTARD: Which I think is the important thing to register. That has already been established.

The second gratuitous comment I would like to make is, it's a strong case, I would think, and I hope you would agree with me, that medical schools should be training their graduates in critical appraisal of such data so that the problem of confusing the errors in this into precise logic is suitably compensated by their understanding of the frailties involved.

THE WITNESS: I think it might be well then for me to reread the remarks I made prior to this discussion, which may help you understand what I get out of this curve.

I said that, "You can see that there is a low, but finite, probability of response at low exposure levels, and that saturation or virtually complete response occurs at high exposure levels. The response at low levels suggests that for all practical purposes there is probably an effective threshold below which clinical asbestosis will be unlikely to occur, although there might be the occasional very susceptible individual who could develop problems.

Since asbestosis is a progressive condition, however, great caution must be exercised in extrapolating these results beyond the timeframe of this study".

MR. LASKIN: That's fair enough.

DR. UFFEN: But to just make this crystal clear, would you be prepared to say whether or not your graph, or things derived from it, could be used with respect to public buildings, schools, distant bystanders or any category...

THE WITNESS: I would refer you to the last paragraph in the text.

DR. UFFEN: Which deals with cancer.

THE WITNESS: It says, "It is likely that the major risk at lower exposures will be to cancer, rather than to asbestosis. I thus believe that studies of morbidity, such as this, should provide only a secondary line of evidence in the consideration of standards for occupational and public exposures.

I think that asbestosis is not a public health problem."

DR. UFFEN: Say the last bit again, because that's not in your written document. "I think that asbestosis is not a public health problem"?

THE WITNESS: No, I believe that asbestosis is almost exclusively limited to occupationally-exposed populations. That's clinical asbestosis.

It wouldn't surprise me if you removed the lungs of everyone in this room and looked quite carefully, you might find some asbestos fibers and you might find a little bit of scarring, but this will have no effect on your health or your life expectancy.

MR. LASKIN: Q. Can you, with all the caveats that you have made in comparing Berry's figure to your figure, I just wanted to focus in on one thing, and you may have touched on it before, is there a difference between Berry's prevalence rate and your probability rate?

THE WITNESS: A. Yes. His prevalence...well, I

5 A. (cont'd.) presume so. I assume that prevalence is the number of cases divided by the number of men. I have taken into account deaths and truncation of followup interval, so they are not exactly the same kind of thing. I do think they can be crudely compared, which is what I've done. It was the only comparison that was available to me, and I thought it was important to see, you know, whether there was any indication that what I was doing was reasonable or was completely way off base.

10 Q. Could you have done Berry's calculation with the statistics that you had?

A. I could have, but I thought this was better.

15 Q. Fair enough, but just to give us some idea, can you tell us if you had done his type of calculation would your figure have been higher or lower?

A. You can do it right now. You can count the number of cases, divide by the number of men in each category, and that will give you the prevalence.

20 So the number of cases listed on the total column in table two, and the number of men at risk either...it's indicated in parenthesis in the first column...and just divide the number of cases divided by the number of men at risk, and that's the prevalence.

Q. One over twenty-four, in the first category?

A. Yes.

25 Q. Fourteen over fifty-six in the second?

A. Yes.

Q. Okay.

30 MR. LASKIN: I was going to move to the mortality study, but if the Commissioners want to deal further with the morbidity study at this point, it perhaps is the best time. Or else we can adjourn for lunch, it being one o'clock.

5 DR. DUPRE: I think you've given me an excellent idea, counsel. If I can just ask this question, again looking at table two: I can take it that the rate of incidence for the zero point nine group is point five, and the prevalence is one twenty-fourth?

THE WITNESS: Yes.

DR. DUPRE: Shall we rise for lunch until two-fifteen?

MR. LASKIN: Two-fifteen.

10 THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: Do you wish to proceed, please, counsel?

15 MR. LASKIN: I was going to proceed with Dr. Finkelstein to the mortality study, but since we seem to be taking Commissioners' Hour a little early, if the Commissioners have any more questions on the morbidity study, it's probably a convenient place to do it and perhaps we should deal with them now.

DR. DUPRE: Do you have any questions, Dr. Uffen, or Dr. Mustard? No.?

20 MR. LASKIN: All right.

MR. LASKIN: Q. Can we, then, Dr. Finkelstein, turn to the mortality study, and just so that we don't have to interrupt you when you are going through it, any more than necessary, I've handed out two separate handouts to the Commissioners and to representatives of the parties, and I take it the first handout is a revised calculation of tables two, 25 three and four?

THE WITNESS: A. Yes, that's correct.

30 Q. Necessitated, I take it, because the number of mesotheliomas in your cohort study was found, on reanalysis, to be ten, rather than eleven?

A. Yes, that's correct.

Q. Best evidence?

A. Yes.

Q. The one, the change...just so that everybody is clear...if one looks at table four, is in group C, the highest exposed group, where the number is now six instead of seven.

Are you with me? One compares...table four is a calculation made upon best evidence, is that right?

A. Well, why don't we...

Q. Do you want to do it when you get to it?

A. Yes, that would probably make it easier.

Q. Okay. Because actually there's nine in table four when I look at, so perhaps we'll wait until we get to it and do it then.

The second set of documents you have given me are copies of certain transparencies which you have, and if you want we'll put them up or we can simply work from the document.

Okay,

A. Okay. Well, in that case we'll then proceed to a relatively-brief discussion of the mortality study at the Johns-Manville plant. This study is still in progress, but some early results are available and I've made these available to the Commission.

Our goals in this study are the same, more or less, as the goals in the morbidity study, and these are case finding for compensation purposes, and the investigation of scientific issues.

Because of latent interval considerations, our initial cohort is again composed of workers hired prior to 1960. I am investigating mortality among all workers with a minimum of twelve months of employment, but because of tracing difficulties, I have split the group into two parts.

I have so far been able to trace most of the men

A. (cont'd.) with nine or more years of employment, and I have prepared the mortality analysis which has been submitted to the Commission.

5 I am currently involved in tracing men from the second group, those with one through eight years of employment. This has been much more difficult, as many of these men appear to have left the province.

10 It is my aim to combine the experience of these two subgroups when tracing is completed. Today, however, most of my remarks concern the long-term employment group.

This group is composed of three hundred and thirty-eight men hired prior to 1960, who were employed for nine or more years.

15 A hundred and eighty-six of these men were production workers in the asbestos areas, fifty-five were maintenance workers. A group of eighty-seven men who worked in the rockwool or fiber glass areas, or who otherwise were minimally exposed to asbestos, served as a control group.

20 I was able to trace about ninety-five percent of these men, and to determine their vital status as of October 31, 1980.

For the purposes of the analysis, all untraced men were assumed to be alive.

25 Analysis of mortality has been done in two ways: Firstly, based upon the official death certificate codings and using the Ontario population an external standard, and secondly, making use of the best information available to make internal comparisons.

30 I found, as have many others, that the death certificate codings or descriptions do not always provide the best information about the underlying causes of death. The best evidence analysis can thus provide useful additional information.

A. (cont'd.) This is particularly true for the mesotheliomas, which are poorly identified in the official codings.

The results of the analysis against the external standard are shown in, I guess this would be table one of the mortality paper...

Q. Tab five.

A. Tab five.

Q. This, again, is based upon death certificate...

A. Death certificates.

Q. And the use of Ontario rates to calculate the expecteds?

A. Exactly.

Let me see if I can find it. Okay.

Q. The expected causes of death are calculated for the purposes of table one, as I understand it, using Ontario death rates for the relevant periods?

A. Okay. The three groups listed in this table are the production workers, symbol P, a combined group of production and maintenance workers who have the symbol P plus M... this group was combined in order to increase the statistical accuracy of the work in order to increase the person-years of observation in each cell.

The third group is the group labelled C, who are the control workers.

There are several noteworthy features about this table. Firstly, mortality is seen to be strongly dependent upon time since first exposure. You may observe that in the asbestos-exposed workers, mortality up to about twenty years from first exposure has been the same as about what would be expected in Ontario males.

Subsequent to that, you can see that there is a rather marked and pronounced increase in mortality as time goes on, from first exposure.

A. (cont'd.) Any analysis which examines short followup intervals, or which merges all of the followup intervals, is thus likely to underestimate the mortality risks.

5 Secondly, you might observe, the control workers have had a mortality experience similar to the general male population of Ontario, while the asbestos-exposed workers have had markedly elevated mortality rates.

10 In the interval twenty to thirty-three years from first exposure, the mortality rates from all causes have been doubled, their cancer rates have been increased by a factor of five, and deaths coded to lung cancer have been eight times more frequent than expected.

15 Looking at the last row, which is the one that deals with heart disease, you can see that asbestos exposure does not appear to have increased the risk of ischemic heart disease among these men.

Q. What figure are you looking at?

20 A. That would be the bottom row, which is ischemic heart disease, and if you look at the twenty-to-thirty-three year column, you will find that there were fewer heart attacks among the factory workers than expected among Ontario males.

Q. What about, just following that, what about the category directly above it, which you call respiratory heart disease?

25 A. Yes. That is a typographical error. That should be nonmalignant respiratory disease, which I corrected on ...

Q. Okay. So that the second last category should read nonmalignant respiratory diseases?

A. Yes.

30 Q. In which you still lump all of the things you told us about before lunch?

A. Pneumonia, asthma, bronchitis, emphysema, asbestosis.

DR. DUPRE: And corpulmonale as well?

5 THE WITNESS: I would have to check the coding guide to see what code corpulmonale gets. Off the top of my head, I can't tell you where it would fit.

Okay? Any other questions about this?

10 MR. LASKIN: Q. Well, if we look at all causes of mortality amongst production workers, I take it then fifty-eight of the hundred and eighty-six production workers have died by the time you did your study?

THE WITNESS: A. By October 31, 1980, yes.

Q. Okay.

15 Do we have an average duration of employment for these production workers? I take it from what you said before lunch that the vast proportion of them are fairly long-term employees?

A. They are all nine years or more.

Q. Even within that category...

20 A. We've got a hundred and fifty-two, or whatever, or fifteen years of more, therefore the difference is those were between nine and fifteen years.

Q. The mesotheliomas...were you going to come to the mesotheliomas, I take it, in more detail, so perhaps I can leave my questions on that for the time being.

25 A. Yes.

Okay, because of the problems involved with the use and modification of death certificate data, I have also used internal comparisons based upon the best available evidence, to investigate cancer mortality.

30 One tumor which is of particular interest is malignant mesothelioma. There have been fifteen deaths from mesothelioma among factory employees, and these are listed on

A. (cont'd.) the top page of the handout which I have given to you.

5 Q. This is entitled, for the record, Mesothelioma Deaths Among Johns-Manville Employees.

MR. LASKIN: Why don't we give these series of transparency copies, which are labelled together as a number, and we'll call them tab seven of exhibit thirty-six.

10 EXHIBIT # 36, TAB 7: The abovementioned documents were then produced and marked.

15 MR. LASKIN: Q. I take it you are now talking, when you have got this transparency in front of you, are not restricting your analysis just to nine year employees, these are all of the employees of Johns-Manville, and these are all of the mesothelioma deaths?

THE WITNESS: A. These are all of the mesothelioma deaths of which I am aware as of today, among anyone who has ever worked at the plant.

DR. DUPRE: This is always at that one J-M plant?

20 THE WITNESS: The one J-M plant.

On this particular sheet of paper I have listed the deaths from mesothelioma, and the various columns in this table indicate firstly the employment period, the year during which the man died, the age at death, latent intervals, and the pathological characteristics of the tumor.

25 The second case on this list is one which has only recently come to my attention, and I consider the diagnosis in this case to be in doubt.

Hopefully the postal strike will soon end and I will be able to obtain some slides for pathological review and clarification of this case.

30 Thirteen of these fifteen cases have so far been reviewed, either by the Tumor Reference Center of the National

A. (cont'd.) Cancer Institute in Ottawa, or by Professor Ritchie of the University of Toronto, and the diagnoses in all these cases have been confirmed.

5 The diagnosis in one additional case, which has required a revision on the tables that I previously gave you, the diagnosis in this case was rejected by the Reference Center, and I'll tell you about this later.

10 There are several noteworthy features about this mesothelioma table. I would like first to draw your attention to the rather disturbing figures in the 'age at death' column. You will note that only one of these fifteen men had reached retirement age by the time of his death.

15 Secondly, you will note that although most of the men had been employed fifteen years or more, several had been employed at the factory for less than two years, and the worker whose diagnosis is so far unconfirmed was employed for less than a month in 1948.

20 All of these men spent at least part of their employment in the pipe plant, and were thus exposed to both chrysotile and crocidolite asbestos.

25 Q. Can we just stop there for a moment?

 Do we know, or have you looked at the question as to whether of these men could have had any asbestos exposure, occupational exposure or otherwise, apart from their employment at Johns-Manville?

30 A. I have only one piece of information and that relates to....let's see...case number six, I guess. On this man's death certificate, his occupation was listed as drywall taper. There is a possibility that as a drywall taper he was exposed to asbestos as well.

 I have no other information about any of these men. Most of them spent most of their working lives at J-M, and probably were unexposed prior to or after.

Q. But for example...

A. Case number one was a truck driver at the time he died. I have no idea what was in his truck, but I don't have any additional information.

Q. Do you have any, you may not have it here, but do you have any breakdown of the amount of time that each of these persons would have been exposed in the cement-pipe operation?

A. No, but I have done the exposure-response calculation which implicitly looks at that.

Q. I take it...is there an exposure profile for each of these people to which we could look and find out how much exposure they had of a combination of chrysotile and crocidolite as opposed to just chrysotile?

A. There are work histories for each of these men. I know exactly what jobs they have done.

Q. So you could get the information?

A. But as I say, they all worked in the pipe plant at one time or another. Therefore, they were all exposed both to chrysotile and crocidolite. I don't think there is any way of quantifying how much of each they might have been exposed to.

Q. Okay.

A. I do mention someplace, in the mortality paper I guess, that I think six of the men worked at the pipe rolling machines where they were probably being exposed to raw fibers of both kinds. One of the men was the machinist, the lathe operator, where he would have been exposed to fiber bound into cement. Another man was a lathe operator who then became a shift foreman and presumably was exposed to raw fiber as well as processed fiber.

Q. This table, I take it, is pretty well current and up to date, by looking at your last case, which is 1981, which is forward in time from the time you wrote tab five?

A. This table is, to my knowledge, as of today.

Q. Can you take this table and tell us which cases from this table now have found their way, first of all into table one, which is your death certificate classifications, and then find their way into table two, which is your best evidence classification?

A. I can't answer the first question. I don't know, or I can't tell you without checking, which of these men were coded to mesothelioma. I can tell you that all of the men with nine or more years of employment appear on table two.

Q. What happened to the difference between the eleven...what happened to the one person who was in your original table two and is no longer...

A. The Tumor Reference Center in Ottawa felt that contrary to the examining pathologist's opinion, this case was not a pleural mesothelioma, but was a primary lung cancer. I have thus altered table two to decrease the number of mesotheliomas by one, and increase the number of lung cancers by one.

Q. And...

DR. DUPRE: Counsel, may we just pause for a moment while I try to ascertain the number with fewer than nine years of employment? They are, respectively, case one?

THE WITNESS: Yes.

DR. DUPRE: Case two?

THE WITNESS: Case two is unconfirmed. It's a possibility. It received the death certificate code for mesothelioma. The description on the death certificate is myosarcoma. However, this man died at the Penticton General Hospital. I don't imagine that the pathologist there is too familiar with diagnosing mesothelioma and I look forward to obtaining some slides from this case.

DR. DUPRE: But whatever the diagnosis, case two

DR. DUPRE: (cont'd.) would not have found its way into tab five?

THE WITNESS: Correct.

DR. DUPRE: So one and two are out, and then nine is out?

THE WITNESS: Yes..

DR. DUPRE: And..

THE WITNESS: Ten is out as well, because he is a maintenance worker and I...

DR. DUPRE: Ten is out? Okay. And then fifteen is out, correct?

THE WITNESS: Correct.

DR. DUPRE: So one, two, nine, ten and fifteen are excluded from tab five?

THE WITNESS: Yes.

DR. UFFEN: May I just ask, the one that was reassigned, how did that come about that it was reassessed?

THE WITNESS: I obtained...this man was autopsied, I obtained the slides from the pathologist at the hospital, I sent them to Ottawa...

DR. UFFEN: You had some question, then, did you, and sent them off to be checked?

THE WITNESS: No. It was my plan to investigate each mesothelioma case...

DR. UFFEN: Each one?

THE WITNESS: ...because it's such a controversial diagnosis.

DR. UFFEN: Have all of them on this list had a double check, then?

THE WITNESS: Every one that says either CTSC or Professor Ritchie has been rechecked. That's thirteen out of fifteen.

Case thirteen, I believe there is some material

THE WITNESS: (cont'd.) available. I haven't got my hands on it yet. And case two, I just found out about just before the mail strike. But this man had an operation prior to his death, so I imagine there is biopsy material available.

MR. LASKIN: Q. With respect to all of the mesotheliomas amongst the employees with a minimum nine years employment, there is only one of them in which the latency period is less than twenty years?

THE WITNESS: A. Yes.

Q. And that's case number three?

A. Yes.

Q. So I take it that explains why when you ultimately get to table four, there are only nine mesotheliomas?

A. Yes.

Q. Because you are dealing with the exposure category twenty to thirty-three?

A. Absolutely.

Q. Okay. But can you tell us, or maybe you are coming to it, what your revision by way of best estimate in your mesotheliomas has done to your lung cancers...from table one?

If anything?

A. It has increased it by one.

Oh, no. Table one is based on death certificates.

Q. Correct.

A. Therefore there has been no change at all because this man actually received the death certificate code for cancer of the prostate, for some reason totally unknown to me and everybody else.

Q. But in going up from seven mesotheliomas on the death certificates in table one, up to ten?

A. Okay. I don't want to confuse death certificates, which are completely unaltered because that's what the coders at Queen's Park have done, with best evidence

A. (cont'd.) information. These are entirely separate.

Q. What's the best evidence information on lung cancer?

A. The best evidence information is the part on the bottom of table two.

Q. Twenty?

A. Twenty.

Q. Okay. And that compares with twenty, right?

A. Yes. It's because several of the mesotheliomas were coded lung cancer, so the number of lung cancers was increased in the death certificate area.

On the other hand, some lung cancers were coded to something else. So when I make both adjustments, the numbers coincidentally happened to turn out the same.

Q. Just to clarify that, when you've made a best evidence adjustment on lung cancer, you've had to take some away and give them to mesotheliomas, but you've added some elsewhere that were otherwise attributed on the death certificates to another cause of death and the end result, you arrived back, fortuitously no doubt, but back at the figure twenty?

A. Correct.

Q. Okay.

DR. DUPRE: In the meantime, are your other figures in table one unaffected, namely the ones for GI cancer and nonmalignant respiratory diseases?

THE WITNESS: All of these figures are based on the death certificate codes which are inscribed in stone. They will never change.

DR. UFFEN: Oh, I remember being told by somebody that you could get it changed if the physician at death agreed to it.

THE WITNESS: Fine. In that case I stand to be

THE WITNESS: (cont'd.) corrected. If someone would care to appeal to Queen's Park, maybe they could change some of these, but...

5 DR. UFFEN: Or am I wrong? I mean I...

MR. LASKIN: That's right.

DR. UFFEN: Is that right?

MR. LASKIN: I think we did hear some evidence to that effect.

10 THE WITNESS: I have no information about that. These are the codes that were written on the death certificates at Queen's Park.

Are there any more questions?

MR. LASKIN: Yes, we have.

15 DR. DUPRE: I have some questions on table one, but maybe I should just put a couple and...but stop me if you are going to cover it later.

THE WITNESS: Okay.

20 DR. DUPRE: The first question I have on table one is, I can take it that your set of figures on all malignancies would be the sum of GI cancer, mesothelioma, lung cancer, plus other malignancies?

25 THE WITNESS: Yes, with one small exception. At least one of the mesotheliomas was coded to this code number 228, which is descriptively called benign respiratory tumor, or something, which is not coded as a cancer. Therefore it would not fall into the 'all malignancies'.

But essentially you are right. That's sort of a minor technical caveat.

30 DR. DUPRE: Well, I'll ask the other question, but probably it is best postponed. But I'll ask it simply so that you can form the judgement on whether it's best postponed.

In a number of the mortality studies that we've seen conducted elsewhere, the tables give numbers exactly as you

DR. DUPRE: (cont'd.) have on table one, but then there is, as is the case in your own tab three, a bracketed figure which is the best evidence figure.

5 If we were to insert some bracketed figures here, do I take it that we would have the number ten in brackets next to the six, for mesothelioma?

THE WITNESS: Yes. Yes.

MR. LASKIN: Q. Have the number nine?

10 THE WITNESS: A. Oh, yes. That's right, nine, because this is twenty to thirty-three years. There would be...

DR. DUPRE: Oh, yes, correct. So the number there would be nine.

15 THE WITNESS: That's right. The number one in the fifteen to nineteen years was the one that was coded 228, I believe, so that was coded correctly.

DR. DUPRE: Would there be numbers in brackets next to the lung cancer figure?

20 THE WITNESS: I thought it would be more fruitful to address this in a separate section rather than to put brackets on this table, which is what I've done.

DR. DUPRE: Fine. That's what I wanted to get at. Please proceed.

THE WITNESS: Okay.

25 Using the best evidence data for mesothelioma and the cumulative exposure data, I've attempted to investigate the exposure-response relationship for mesothelioma in two ways. The first way was the prospective cohort approach, and this is discussed in the paper submitted to you.

30 In this approach, I divided the long-term production workers cohort into three groups, ranked by cumulative exposure, and calculated mesothelioma rates in each group. The results are presented on the next graph on this little package.

THE WITNESS: (cont'd.) The rates are, of course, quite unstable because of the small numbers involved. There appears to be a clear trend to increasing risk with increasing exposure, and the results appear to be compatible with the linear relationship.

Have you any questions about that?

MR. LASKIN: Q. You are there plotting, I take it, the results which appear on table four?

THE WITNESS: A. Yes.

Q. For mesothelioma, the three points...

A. Yes, that's right.

Q. ...on your graph are the three mortality rates that show for mesothelioma in table four?

A. That's right, plus I've thrown in at the origin the Ontario rate, which is essentially zero...which gives me four points to fit.

Q. All right.

A. Okay. The second approach that I used was the case control method, which allowed use of more of the mesothelioma cases. I was able to use the cases with fewer than nine years of employment in this approach.

In this technique, thirteen of the fifteen mesothelioma cases I showed you, with the exception of cases number two, which is the unconfirmed case, and case number ten who is the maintenance worker, each of these cases was matched with four workers known or presumed to be alive after the death of the index case.

This calculation is still, in a sense, preliminary, because eleven of the fifty-two controls, or twenty-one percent, are still untraced. The results are presented on the next table.

As you can see, the controls were selected according to the following criteria: They were the first four men in alphabetical sequence starting from the index case, who

THE WITNESS: (cont'd.) were first employed in the same year, plus or minus one, were born within four years of the index case and were still alive when the index case had died.

The results of this analysis are again compatible with the dose-dependent cancer risk.

MR. LASKIN: Q. I think you are going to have to help us a little with this.

Have we all got the table, to start with?

DR. DUPRE: Table four?

MR. LASKIN: No. We are now on...

THE WITNESS: It's called mesothelioma...

MR. LASKIN: We are now on tab seven, which is the copies of the transparencies, and if we go to the third page, I take it this is where your case control analysis is?

THE WITNESS: Yes, it's a table entitled Mesothelioma Case Control Analysis.

MR. LASKIN: Q. Now, the controls are these workers who are or were exposed to asbestos?

THE WITNESS: A. These were workers who were employed for a minimum of twelve months and had a minimum of twelve months of exposure.

Q. To asbestos?

A. To asbestos. They may have been employed for twelve months plus one day, but they would have qualified for admission.

Q. Under your selection criteria?

A. Yes.

Q. Can you tell us how you calculate these relative risk figures and what this table represents?

A. Yes. Essentially you select a reference group. It's usual to select the ones with the minimum exposure. In this case, zero to forty-nine fiber years.

You count the number of cases and the number of

5 A. (cont'd.) controls in this group. You then presume that if there were no dependence between mesothelioma risk and exposure, the proportion of cases and controls should be the same in each of the other groups.

Q. In other words, roughly one for eight?

A. One for eight.

10 As you can see, as the exposure increases, the number of controls becomes fewer in comparison with the number of cases, which implies that there is a causal relationship between exposure and result.

Okay.

15 The numbers are very small, which explains why I think the fourth category has a risk which is higher than the subsequent one, but, you know, I think the essential thing is that there is a trend which is compatible with higher risk at higher exposure.

Q. Just so that we are all clear, let me see if I can go through this again in my own layman's way.

20 You have these thirteen cases of mesothelioma which are listed here, and you then took four controls for each case randomly according to your selection criteria, and then having got your fifty-two controls, I take it, you then looked at what their exposures had been?

A. Yes.

25 Q. To asbestos, and you then allocated them to your various exposure categories?

A. Yes.

Q. Then you took your minimum exposure category and assumed that that had a relative risk of one?

A. Yes.

30 Q. The ratio of cases to controls to produce a relative risk of one was roughly one-to-eight, and then you

Q. (cont'd.) measured the ratio of cases to controls for all the other exposure groups against that relative risk of one?

5 A. Yes. If you like, I can just write a formula on the board.

Q. Why don't you, and that will assist us.

10 A. Okay. If you just draw a little table, say this is exposure category A, exposure category B, if this is case control and if we've got little a, little b, little c, little d for the number of men in each of these categories, and by definition the relative risk is c over d, over a over b, and that's how these are all calculated.

15 You assume that the proportions should be the same in all the other groups, and if they are not, then this is how you calculate the relative risk.

Actually, I may have this upside down, but in any event, that's the general idea.

20 Q. I think if we go back and look at Dr. Sackett's address, I think in his appendix he has precisely that calculation for relative risk when you are using a control group.

But I take it the thrust of all of this is that you approach this whole question of exposure response with respect to mesothelioma both by means of a case control analysis and by means of a cohort analysis, and roughly, basically it comes to the same conclusion?

25 A. Right. One often hears quoted or sees in print the statement that because, you know, someone with one day's exposure has developed mesothelioma, there is no dose response and this is hazardous at all levels.

30 My feeling is that you may certainly develop a mesothelioma with a short exposure, but the risk is substantially less than the risk at higher exposures, and I think both these analyses are consistent with that.

5 A. (cont'd.) You know, the case who has developed mesothelioma after a week of exposure has had four hundred thousand, perhaps other people, who have also been exposed for a week and have not developed mesothelioma, so, you know, an anecdotal case does not establish that there is a high risk, whereas in the high exposure categories here, you know, we've got six cases out of sixty-two men. That's a risk, whereas in the general public, if Joe Public with no known asbestos exposure or with a neighborhood exposure develops a mesothelioma, and 10 he has had four hundred thousand neighbors, then his risk is one in four hundred thousand compared to six in sixty-two, and that's obviously different.

15 So, you know, I think this has public health implications and both of these analyses, which are complementary, I think, show that there is a dose-dependent risk.

20 Q. Do we know how many of your cohort of persons, production workers...or indeed maintenance workers...were exposed only to chrysotile would fit within your selection criteria?

25 A. Certainly before 1955, they were all exposed to both. Anyone who worked only in the board shop after 1955 would probably have been exposed directly only to chrysotile, but crocidolite was lying around in bags at the plant.

30 Unfortunately, the latent interval is still too short to look at these men. If you look at the first table you will see that the latest that a man dying of mesothelioma was first employed was May, 1955. The flex board shop opened in July, 1955, so not enough time has elapsed to be able to draw any conclusions from that.

Q. On the question of fiber type and its effect on mesothelioma?

A. Yes. At this plant.

Q. At this plant.

DR. DUPRE: Dr. Finkelstein, could I just ask

DR. DUPRE: (cont'd.) this question? Why do you have thirteen mesothelioma cases in this table we have just been looking at?

THE WITNESS: I excluded the man whose case was unconfirmed, because I just didn't think it was a reasonable thing to do to throw him in.

DR. DUPRE: Which one was that? Was that...

THE WITNESS: That's case number two, I guess.

MR. LASKIN: Number two.

DR. DUPRE: Case number two.

THE WITNESS: I also excluded the maintenance worker because I have no way of estimating what his exposure might have been, and I couldn't have thrown him into any of these categories.

DR. DUPRE: And that is case number...?

THE WITNESS: That is case number ten.

Any more questions at this point?

MR. LASKIN: Q. I want to go through with you your quantitative risk assessment, but I guess that's best left until you have dealt with the remainder of the matters in the paper.

THE WITNESS: A. Okay.

Given the past mortality experience of these workers, it is possible to make some predictions about what might happen to the cohort in the future. And by cohort I mean here the nine year production workers.

Geoffrey Berry has noted that there appears to be a power function relationship, a mathematical term, between mesothelioma incidence and interval from first exposure. I have compared mesothelioma incidence rates among the long-term production workers with those observed by Berry in his British factory cohort, and Selikoff among his American and Canadian insulators cohort.

The results are shown on the next sheet of paper,

THE WITNESS: (cont'd.) which is a graph.

The data here is plotted on log log graph paper. This is a special kind of graph paper on which a power function relationship appears as a straight line. I have drawn lines through these data points by eye, and within the limitations of the data the lines appear to be both straight and parallel, and the incidence rates are roughly proportional to the cube of the latent interval.

It's a little formula that I have written down halfway down the sheet of paper.

I have concluded from this that the time course of mesothelioma incidence has been roughly the same in all three of these populations, and that one might thus anticipate the Johns-Manville cohort to have an analogous experience in the future.

In the bottom corner of this graph I have attached an illustration from Selikoff's paper, in which he has plotted mortality rates against time from first exposure, and I have drawn in on this little insert the maximum Johns-Manville interval to date.

From this comparison, it would appear that we would be seeing more mesotheliomas deaths among these men in the future. Advances in medical knowledge will be required before we can intervene to alter this prediction.

MR. LASKIN: Q. Can we just dwell on this sheet of paper so we all get it clear, and you are going to have to explain to me as a layman what a power function relationship is.

THE WITNESS: A. Okay. Basically, if you look halfway down, I've scribbled a little thing which says I, and a funny little symbol which is mathematicalese for 'is proportional to' or 'related to', time essentially, raised to the third power. The power function relationship says that two variables are related through some sort of relationship like this where you raise one of the variables to a certain exponent.

Q. Is this what is meant by incidence is inversely related to the cube root of time since first exposure?

A. Not inversley, but directly related.

Q. Directly related to the cube of time since first exposure?

A. Right. The cube comes...let's see if I can explain it to you from this paper...basically the exponent is the slope of this line, and if you look you will see that the line rises three units for every one that it goes across, so the slope in mathematical terms is essentially X over Y. Here X is three times Y, which is a cubic relationship.

I don't think that's crucial here. I think the important point is that this population seems to be having the same experience as the other two that we know about.

Q. Can you just tell me, how did you know where to plot the points to get your line?

A. I looked at the incidence rates in each of the intervals - fifteen to twenty years, twenty to twenty-five years, twenty-five to thirty years. That's table...

Q. Can you go slowly on this?

A. Yeah, that's table four.

Q. Okay. Let's go slowly here. Table three or table four?

A. Table four.

Q. Table four?

A. If you look at the line that says mesotheliomas, I quote mortality rates...wait a minute. I take that back. It's not where it comes from.

Okay, allow me to start over from the beginning.

Q. Okay.

A. What I did was I looked at the number of deaths occurring in each of the intervals - fifteen to nineteen years, twenty to twenty-four years, twenty-five to thirty years,

A. (cont'd.) thirty to thirty-five years, counted the number of mesothelioma deaths in each of those boxes.

I then calculated the number of person years at risk in each of those boxes, and divided the two numbers.

Q. And that gave you your...

A. That gave me the four points to plot on this sheet of paper.

I made a similar calculation from the raw data Selikoff presented in his paper, and Peto had already digested the Newhouse and Berry data in his Lyon paper to give me his data points.

Q. Would I be correct...and I don't want to confuse this...but am I correct that you basically made the same kind of incidence calculation that you did in table two of tab four?

A. Yes. Time specific for each of these time intervals.

Q. Time specific?

A. Yes.

Q. Okay. And in the little box in the...I'm sorry, Dr. Uffen...in the little box in the corner, I take it, is Dr. Selikoff's data and you have just, you have drawn a vertical line...

A. That's right.

Q. ...at your maximum cutoff point of thirty-two years?

A. That's right.

DR. UFFEN: Were you about to finish with that?

MR. LASKIN: No, no. You go ahead though.

DR. UFFEN: Maybe I'm just anticipating, but were you going to discuss the implications, if any, of extrapolating any of those three curves towards the origins?

THE WITNESS: We don't have to extrapolate towards

THE WITNESS: (cont'd.) the origin because we know what the experience is at shorter intervals of time. There are no observed deaths earlier than...well, actually there is one
5 observed death, number one on the table I gave you, whose latency was only fourteen years.

However, I don't know the...since I haven't traced all the briefer-term employees, I don't know what the person years of risk are in that group.

DR. UFFEN: Let me put it this way, and you tell
10 me whether this is a misuse of the data: If you were to project data, the Selikoff or the Newhouse and Berry curves, back to incidence one, it would intersect at somewhere around fifteen to seventeen years?

THE WITNESS: I think if you look at the insert you see actually what happened to Selikoff's cohort.
15

DR. UFFEN: What about yours? If you did the same thing with yours? If you projected back, it would be...

THE WITNESS: I don't see much reason to project it back because this is something we can observe directly. I think projecting forward may make some sense because it will
20 enable you to make a prediction, but I've already observed everyone through fifteen years and I know exactly what has happened to this cohort. So there is no projection back in time that's required. It's a projection forward in time.

DR. UFFEN: But suppose you are trying to understand the differences between your curves and Selikoff's, and Newhouse and Berry's?
25

THE WITNESS: My observation is that there is no difference. The absolute rates are different because I've selected a higher-risk group.

Theirs are diluted by people with shorter-term
30 employment.

So essentially the question I'm asking when I

THE WITNESS: (cont'd.) draw this is, is the time behaviour of the Johns-Manville group similar to the time course that we have seen...

5 DR. UFFEN: That I understand in the point you have made, but is it possible to go further and ask other questions, and you've given an answer that you have selected a different group.

THE WITNESS: You propose the question and I'll see if it can be answered.

10 DR. UFFEN: Well, suppose someone else comes along and makes a study of another different risk group, very low-risk group, like school children in a school. Would their curves be the same or not?

15 THE WITNESS: That's basically a question about the biology of mesothelioma. I don't know how to answer that question.

20 MR. LASKIN: Q. Do I take it from your previous comments to Dr. Uffen that you don't place any particular reliance on the fact that your line is to the left of the Newhouse and Berry line, or to the left of the Selikoff line? You are attempting to show a trend.

25 THE WITNESS: A. The question I asked myself is, is that little formula that I've written down, which is applicable to the two other groups, applicable to the Johns-Manville group as well. The answer is yes, if the line is straight and parallel to the other two, which it is. I therefore answered the only question I was interested in asking. I have an explanation as to why the rates are higher, which is observable because it's displaced to the left, and that is because there is less dilution by low-risk people in my nine year group.

30 When I throw in...throw in is a poor term to use... when I merge the experience of the people with...

Q. One to eight.

A. One to eight, I expect the curve to shift over and meet theirs.

Q. And be more in line with theirs.

5 If you did a calculation such as Dr. Selikoff did and you have shown in the lower righthand corner, would you expect to see the kind of curve that he has got there with respect to your data?

A. The problem is, I've only got ten cases here.

Q. Right.

10 A. Now, he has had a hundred and fifty or so. The reason I made this calculation was to be able to make use of his much larger experience, okay? I could plot the same kind of thing, but in the small number the problems would be so large that I don't think it would be a profitable experience.

15 Q. I take it you could do it by translating your incidence rate to person years at risk and just...

A. Yes, absolutely.

Q. ...and presumably since the one is going up, the other would go up as well?

20 A. Actually, if you look at table three...no, no, I take that back.

25 Well, see what he has done is he has merged...he has no exposure estimates. I have done this calculation, but I've broken it up by exposure categories, so essentially I've done that in table three where I've done it by age, and table four where I've done it by exposure. But it's not comparable to his because he lumped them all together.

Q. What does table three show?

30 A. Table three shows the mortality rate by age at death, for mesothelioma at the top, lung cancer at the bottom, with the Ontario lung cancer rates appended at the very bottom of the page.

Q. So that looking at mesothelioma, two persons

Q. (cont'd.) died of it in the age category thirty-five to forty-four. There were four hundred and thirteen man years at risk in that age category, and you've therefore
5 got a rate of four point eight per thousand man years.

Is that all correct?

A. Yes.

Q. Similarly for the rest of the age categories?

A. Yes.

Q. In table four, you've addressed it in terms
10 of dust exposure, or fiber exposure?

A. Yes.

Okay, well, I would like to...

DR. DUPRE: On table four I still have one
question to clarify it.

What are groups A, groups B and groups C again?
15

THE WITNESS: I've got a hundred and eighty-six men. I divided them into three groups of sixty-two men each, the men were ranked from one to a hundred and eighty-six, based on their exposures. I chopped them into three groups of
20 sixty-two, and these are groups A, B and C. In the text of the paper I tell you what the mean and standard deviation exposures were in each of these groups.

MR. LASKIN: For the record, I think that's at
page thirteen in the second full paragraph.

DR. DUPRE: In tab five?

MR. LASKIN: In tab five, and just to clear the
25 record, the dust categories you chose, which are eight to sixty-nine, and sixty-nine to one twenty-one, and one twenty-two to four twenty are unusual so that you could have an even number of persons in each dust category.

THE WITNESS: A. Yes. Rather than select the
30 dust category, I selected the group size and the dust categories fell out.

Q. Sixty-two to each group, a total of one hundred and eighty-six production workers?

A. Right.

Okay, I would like now to say...

DR. DUPRE: And groups A, B, and C, therefore, are an increasing order of exposure?

THE WITNESS: Yes, that's right.

Okay, I would like to say something briefly now about the other cancers. As I've showed you on table one, lung cancer death rates have been markedly increased.

I have attempted an exposure-response analysis for the long-term employees, but the relationship does not conform to what one might anticipate, and this is apparent on the last graph in tab...whatever it is...five, I guess, where you can see rather than increasing nicely, the last exposure category takes a dip.

There are several possible explanations for this. Firstly, the numbers are small so that random fluctuation may be distorting the pattern. Secondly, smoking habits might be playing a confounding role, or thirdly, my exposure assignments might be inadequate.

It remains to be seen what will happen to the exposure-response relationship when the short term employees are merged into the analysis.

Mortality rates from gastrointestinal cancer were increased among the asbestos-exposed employees, but numbers were so small that statistical significance was not achieved.

Table four, now that you have it in front of you, indicates that rates increased with increasing exposure. This is suggestive of a cause-and-effect relationship, but does not prove one.

Q. Can we stop there before we go to the next

Q. (cont'd.) part, because what I think it might be appropriate to do is deal with your risk assessment in some detail.

5 Could I therefore, perhaps easier for all of us, if we go to page sixteen of the text, and seventeen.

 Let's start with the formula which you used, and is this a formula, I take it, that you developed yourself?

A. Yes.

10 Q. Okay. Now, can you tell us the thinking behind the formula and why you treated mesothelioma in the way you have and why you treated lung cancer in the way you have?

15 A. Well, the first thing that struck me in looking at the data was that it was primarily the younger men who seemed to be dying of mesothelioma, and the older men dying of lung cancer.

 So I was interested in finding some sort of explanation for this phenomenon.

20 Julian Peto in his Lyon paper, I believe, or possibly elsewhere, has suggested that age is not an important variable in terms of mesothelioma risk, that the primary factors are intensity of exposure and latent interval.

25 I thus decided to try a model in which mesothelioma was independent of age. It has been observed in several populations that the lung cancer risk among asbestos workers seems to be a multiple of their natural background rate. So I thus constructed this little model whereby the total risk was the sum of the risks from mesothelioma and from lung cancer, in which the mesothelioma risk was independent of age but in which the lung cancer risk was dependent upon the background lung cancer risks in the population.

30 This model is nice because it explains why we are seeing mesotheliomas in the younger men and lung cancers in the older men. The background rate for lung cancer is

A. (cont'd.) very low in young men. We, therefore don't expect to see very many according to this model, in younger workers, and indeed we don't.

5 On the other hand, the lung cancer rates in Ontario increase almost exponentially with age, so that the background rate among older men is quite high. Therefore, if there is a multiplicative effect of asbestos exposure, as the men get older we will be seeing more and more lung cancers, and if the mesothelioma rate is independent of age and constant, 10 it will be swamped by the strongly-increasing lung cancer rate, with the net result that if you look at the whole group you see mesotheliomas among the younger men and very few lung cancer, you see a few mesotheliomas among the older men, but most of the deaths will be due to lung cancer among the older 15 workers.

Q. I don't know whether you were here for Dr. Nicholson...

A. No.

Q. ...and I can't ask you the question, but it seemed to me as you were explaining it that the rationale for your theory seemed to fit with the kind of evidence that he was suggesting, but I won't ask you about that since you weren't here.

A. Again, this is a statistical model and may or may not have anything to do with biology.

Q. All right.

A. It fits the data, but it's purely...

Q. All right. Can we then...I just want to go through your calculations as to what you've done, and let's start with mesothelioma and then we'll come to lung cancer.

30 Okay, you have, I take it, for your calculation with respect to mesothelioma, dealt only with the most exposed group, the C group?

5 A. I've done it two ways. In a section of page sixteen, labelled mesothelioma, I dealt with only the C group. However, since the exposure-response relationship appears to be linear, I could have dealt with any of the groups or I could have used the approach I used in part B, which was to merge them all and use the group average.

Q. Okay.

10 A. So I've done it in two ways, in essence. If the exposure-response relationship is linear, these two ways are completely equivalent.

Q. Okay. Let me see if I can understand the first way you've done it. You've gone to group C, which is at...let me get the tables here...

A. It's on table four.

15 Q. Which is at table four, and that shows an incidence rate of thirteen point nine, which I take it is fourteen deaths per thousand men, is one way of looking at it?

A. Right, now corrected to twelve.

20 Q. All right. We'll come back to the correction, but I think it's easier if we work at the text. So that fourteen deaths per one thousand man years of observation is what you worked from, and they have...those persons in the most exposed group they're in a one twenty-two to four twenty fiber category, with an average fiber exposure of a hundred and eighty?

25 A. Yes.

Q. Okay. Then you say you take a group of a hundred men, starting employment at age twenty-five, and you then asked the question, how many mesothelioma deaths might we expect between the age of forty-five and sixty?

A. Yes.

30 Q. I take it then the ages at risk are forty-five to sixty, so that those one hundred men would have

Q. (cont'd.) fifteen thousand man years at risk?

A. A hundred times...

Q. Fifteen hundred.

A. Fifteen hundred man years.

Q. Fifteen hundred man years at risk?

A. Yes.

Q. Being a thousand for the age forty-five to fifty-five, and five hundred for the age fifty-five to sixty?

A. Yes.

Q. Okay. So that fourteen deaths in a thousand man years at risk translate to twenty-one deaths in fifteen hundred man years at risk?

A. Yes.

Q. Do you make any correction for the fact that some of the people may have died in the interval?

A. No. I certainly considered that but I consider that to be a second order correction, again in mathematical terms, given that this is a rather crude calculation to begin with, that's icing on the cake.

Q. Then assuming a linear dose-response relationship, in order to find out what the risk would be at one fiber per c.c., what do you do?

A. Well, the average exposure at a hundred and eighty fiber years for eighteen years is ten fibers per c.c. At one fiber per c.c, the risk would then be one-tenth as great, so one simply divides by ten.

Q. So it's two instead of...

A. So it's two instead of twenty-one.

If you make the correction that the death rate was only twelve per thousand man years instead of fourteen, then you still end up with two when you divide by ten.

However, if you don't divide by ten, the line above that should then read, then we might expect eighteen of these men

A. (cont'd.) to die of mesothelioma between the ages of forty-five and sixty.

Q. Sorry?

A. Okay. It's the one, two, three, four, five, six...the seventh line down.

Q. Okay, your corrected number of mesotheliomas, ten instead of eleven?

A. Eleven, leads to eighteen instead of twenty-one on that seventh line down.

Q. Okay. But still leads to a rough estimate of two at one fiber per c.c.?

A. Yes.

Q. Now, can we go to your calculation of mesothelioma and lung cancer, and I take it now instead of.. I'm sorry, Dr. Uffen.

DR. UFFEN: Were you going to go through the rest of that paragraph? There's only eight more lines to go...

MR. LASKIN: Sure. Yes.

DR. UFFEN: ...to pick up a point that we bypassed this morning.

MR. LASKIN: It's a good point. Okay.

MR. LASKIN: Q. Then you've also...and I think fairly, Dr. Finkelstein...considered the possibility that you may have underestimated your dust exposures?

THE WITNESS: A. Yes.

Q. By even a factor of ten. And how do you then correct for that?

A. You simply divide by an additional factor of ten, and I've done that by considering a thousand men instead of a hundred.

Q. So it would be point two men for a hundred, or two men per thousand?

A. Two men per thousand, yes.

DR. UFFEN: This seems to be an opportune moment for me to raise one little issue that we never settled.

5 With people who were employed twenty or thirty years ago, would it have affected your study at all that they might have had a longer working week, or worked overtime, and that sort of thing?

THE WITNESS: In principle, yes. In practice, the uncertainties are so large anyway that this is a third order of correction.

10 I should point out that apparently many of these men did work overtime. The plant worked six or seven days a week, some of them came in during their supposed days off to help in maintenance and whatnot, so that the work week was longer and many of these men did work overtime.

15 DR. UFFEN: I know you only put this in as an example that we postulate that we've underestimated by a factor of ten, but is that a reasonable possibility that estimates could be out by...

20 THE WITNESS: I actually, in the text of the papers, guessed that my estimates were accurate within a factor of three to five.

My science background shows me a selective factor of ten because it's a pretty number for this particular calculation.

25 MR. LASKIN: Q. All right. Can we then go to your group calculations which are mesothelioma plus lung cancer?

THE WITNESS: A. Yes.

Q. Here, I take it, with respect to, for example mesothelioma, you deal not simply with group C, but you deal with the average of groups A, B, and C?

30 A. That's right. As I pointed out before, since the dose-response is linear, it's exactly the same and makes no difference.

Q. Okay. And what you've come up with is an average exposure, cumulative exposure for these hundred and eighty-six production workers of a hundred and five fibers per c.c.?

5 A. Yes.

Q. Now, your detailed calculation, I take it, is at page twenty in appendix two?

A. Yes.

Q. I take it what you have is a hundred and five fiber years per cubic centimeter, translates roughly into six fibers per c.c.?

10 A. Correct.

Q. You then have ten deaths...now these are the uncorrected ten deaths because we have excluded the one that occurred earlier...so these are ten deaths twenty to thirty-three years after first exposure?

15 A. Yes.

Q. A total of thirteen hundred and thirty-seven man years at risk produced those ten deaths?

A. Yes.

Q. So that if you then standardize that to a thousand man years at risk, you get your seven point five deaths?

20 A. Yes.

Q. Okay. Now, what do you then do to bring that figure down to what you would expect at one fiber per c.c.?

A. Again, we've based on this model the average exposure was six fibers per c.c. for eighteen years. In order to bring it down to one, I simply divide by six, which brings the risk down by a factor of six as well.

25 To calculate the number of deaths you then multiply the annual risk times the number of man years at risk, which for a thousand men between the ages of forty-five and sixty would be fifteen thousand man years of risk.

30 Q. A thousand men between forty-five and sixty

Q. (cont'd.) would be fifteen man years at risk for each man?

A. Correct.

Q. So it's fifteen thousand, again assuming that nobody drops out along the way?

A. Correct.

Q. That's how you get your figure of nineteen?

A. Correct.

Q. Okay. Let's now go to the lung cancer calculations and can you first of all tell us how you get your constant, how you get your figure C? What do I look for to get my figure C?

A. Well, if you look at table three, compare the lung cancer mortality rates in each of the age groups with the Ontario rates, 1970 to 1974, which I must emphasize, the Ontario rates quoted here are based on death certificate data from Queen's Park. The lung cancer rates are best evidence data, so that they are not strictly comparable.

However, the modification in the Ontario rates, if they were based upon best evidence data, might be ten or twenty percent, which is very small in the context of this kind of calculation.

So basically, if you look down the various columns it would appear that the Johns-Manville lung cancer rates have been about ten times, eight to ten times higher than the Ontario rates. So I've arbitrarily selected eight as the multiplication factor, and therefore, if one plus C is equal to 8, C is equal to 7.

Q. Okay. Then in order to find out what the rate is at one fiber per c.c., you must then take your constant C, divided by six, add one to it, and multiply the whole thing by the Ontario death rate for the particular age group you are talking about?

A. Correct.

Q. And you've got two different age groups.

You are talking about nineteen...I guess the forty-five to...what is it?

A. Forty-five to fifty-four, and fifty-five to sixty.

These categories are selected because that's the way the Ontario rates are tabulated.

Q. So that having done the calculation, if we are looking at appendix two, little two R, at one fiber the risk will be, I take it, two point one plus how much? Plus three point two, I believe, is that right?

A. It will be two point one times one point five for the older men, and two point one times zero point five for the younger men.

Q. Okay. Those are...so those are the risks at Johns-Manville, and you then multiply those by the man years at risk, which is ten thousand for the first age category and five thousand for the second, and you come up with the figures of eleven and sixteen?

A. Yes.

Q. What are they corrected?

A. Corrected, there is really no change because I've guessed an eightfold increase, and the change of one man doesn't modify that at all.

Q. Has ...having gone through all of this...has, to your knowledge, anyone else in the literature, the epidemiological literature of which you are aware, has anyone else done this kind of risk assessment?

A. Julian Peto has attempted it, and he provides one data point for lung cancer, based on a linear dose-response model. In his British factory cohort, which consisted of men first employed at 1951 or later, there were no mesothelioma deaths,

A. (cont'd.) so he was unable to make a calculation for mesothelioma.

5 So essentially there is one data point in the literature, which is Peto's.

Q. What do you mean by a data point?

A. He says the lung cancer risk is doubled at two hundred fiber years. So if you are drawing a graph of risk against dose, there would be one spot on the graph, and that's it, a factor of two hundred.

10 Q. Now, I take it that one of the matters you have looked at is whether any of the other cohort studies that are available and around are, apart from Dr. Peto's, are useable or can be employed to develop a quantitative risk assessment model?

15 A. Yes, that's right. I've prepared again a transparency, which we'll dispense with, but there are some hard copies.

Q. Why don't we distribute that and discuss that for a moment.

20 All right, let's give this a number first. It's tab eight of exhibit thirty-six.

EXHIBIT #36, TAB 8: The abovementioned document was then produced and marked.

25 MR. LASKIN: Just for the record, before I forget about it, I'm not sure we marked those revised tables, did we?

MS. KAHN: No, we didn't.

MR. LASKIN: Would you ask Mr. Casgrain?

MR. HARDY: He has ceded his interest to me, for the time being.

30 MR. LASKIN: All right. Let's call it tab nine. That's your revised tables, just so we don't lose track of it.

EXHIBIT # 36, TAB 9: The abovementioned document was then produced and marked.

MR. LASKIN: Q. Can you tell us what you have done here, on tab eight?

THE WITNESS: A. What I have done is, I have listed all the studies that I'm aware of which attempt a dose-response analysis for mortality, with the exclusion of the Quebec mining study because I don't think that's particularly relevant to the Ontario experience from a quantitative point of view.

Q. Just stopping there, why do you say that?

A. There are probably...well, there is possibly one asbestos mine operating in Ontario. It doesn't employ very many men. If one were to consider a standard for Ontario asbestos mines and mills, I would suggest that the McDonald data be used unmodified - that is, based on impinger data - as he suggested in one of his papers himself.

I don't think that it...well, we've heard from every witness that has appeared before us that there is an increment in risk between...from the mine to secondary processing, so what we are concerned about in Ontario are these secondary processing situations.

Since McDonald's data cannot be converted from total dust to fibers, since it is not relevant from an occupational point of view to Ontario, I've omitted it from consideration here.

The other four mortality studies are Enterline's Johns-Manville retirees study, Hans Weill's asbestos-cement study from New Orleans, Julian Peto's Rochdale study, and John Dement's textile study.

So those are the four studies, and the only four studies, which are available in the world literature.

5 A. (cont'd.) For each of these studies, I've made some very brief comments upon what I consider possible drawback to be, and then I've made my own personal assessment as to the use that can be made of these studies.

As you can see, in my estimation Peto's study is the only one which provides any data for a quantitative assessment, and that deals solely with lung cancer.

10 Q. I think we've no doubt, we've had a lot of evidence, or some evidence, on the one and two, but can you elaborate your comments with respect to the Dement paper? Can you elaborate upon those comments?

15 A. Yes. Dement has used a methodology to calculate his exposure-response relationship which is unorthodox. What he does is he divides his exposure into various categories, as I have done and as others have done, but rather than leave a man in an exposure category, as the man continues to accumulate exposure he moved him from one exposure category to the next... leaving behind, however, the man years at risk that each man has contributed to each of these categories.

20 This, I think, would be a reasonable procedure for a disease that killed you in a week or two. If we are talking about chronic disease and we are talking about mortality from diseases which take twenty-five or thirty or forty years to develop, I think this is not an appropriate procedure.

25 A man will die in only one of these categories, but he has contributed his risk to the others.

Q. Can you explain that, because I think it would be helpful to us all if you could illustrate that.

30 A. Okay, essentially what one is concerned about is risk, which very crudely I'll define as the number of deaths divided by the person years at risk in any exposure category. So if you take a...draw a graph, and on the Y axis you place exposure category, which we'll just crudely call A, B, and C, then

5 A. (cont'd.) in order to calculate a dose response, you want to count the number of deaths in each of these boxes and divide them by number of years at risk in each box, so this would be $N \text{ sub } A$ over person years of risk in box A, and B over person years at risk in box B, etc.

Q. Hold on. Let's just go slowly here.

10 Could your A, B and C's you've got there correspond to the A, B and C groups that you have in your mortality...

A. They could correspond to anything. These are just symbols.

Q. They could be cumulative exposures?

A. Yes.

Q. And your N's are what?

15 A. N is the number of deaths.

Q. All right.

A. Okay?

20 What I have done is to fix who enters each of these categories, A, B and C. I start looking at twenty years after exposure and look forward in time. Okay? So a man starts off in one of these boxes, and we see what happens to him.

25 What Dement has done is, he has defined his exposure ranges here, whatever they may be, so many thousand man days or fiber days, whatever, but that's irrelevant. As a man...so every man will start off in category A, which is very low exposure.

If he lives long enough and continues to work, and accumulates exposure, he will move into category B.

30 However, Dement starts counting person years at risk from the moment that this man enters the box. Okay? So...

Q. Without any account for latency?

A. Without any account for latency.

A. (cont'd.) So, suppose there were no deaths here. There would be person years at risk contributed by everyone who has moved into group B.

5 Suppose...I could give you a ridiculous example... suppose everyone dies two days after they make it into group C. When the number of deaths in group C will be everyone in the group, the number of person years at risk will be two days times however many men, which would be close to zero, the risk will
10 be N over a very small number, which will be fantastically large.

 At the same time, there are no deaths in this group, and there are no deaths in this group, so the risk here is zero.

 However, we are talking about diseases which may have started fifteen years previously when the man was in this box or this box, and which have taken fifteen years to develop,
15 and he dies in this box.

 So effectively what this does, this calculation approach, is because you are accumulating your denominator, the person years of risk, in these categories, you are accumulating most of your deaths in the higher categories, what you are effectively doing is you are underestimating the risk at low
20 exposure categories, overestimating the risk at high exposure categories, and automatically manufacturing a linear...well, not linear, but a dose response which is low down here and high up here.

 Okay, so from that point of view I don't think
25 that Dement's data can be used in a quantitative fashion. I think his study may have very great merit if he redid his calculations.

 He has got exposures, he has got responses. If he recalculated them in some more acceptable fashion, this would provide more data for a very limited pool of quantitative
30 information.

Q. Can I try in my own way to understand and see if I understand all of what you have just been telling me, by asking you whether I am correct that you, yourself, have corrected
5 for the deficiencies that you've just outlined, in two ways. The first way is that you cut off your cumulative exposures at eighteen years?

A. Yes.

Q. Secondly, you then only looked at these people
10 with a latency period built-in, which was eighteen, twenty years from first exposure?

A. Yes.

Q. And Dement has done neither of those things? So far as you understand the paper?

A. Well, Dement in part of his paper looks at
15 latency, okay? But that is not the part where he talks about dose response. When he talks dose response, he does it this way.

Q. People are accumulating man years at risk from their very first entry into the work force, and...

A. Well, actually I think it's from ten or fifteen
20 years after, but I think the same criticism applies because we are dealing with chronic disease. If we were dealing with carbon monoxide poisoning where the effect occurs the same day, then you don't have to worry about what happened ten years previously.

It's a technical point which, unfortunately, I think invalidates the dose-response calculation, which
25 unfortunately is exactly the same technique that was used in the uranium miner studies in the States, which I think invalidates the uranium miner risk estimates as well.

Q. In other words...let me try another question to make sure I've understood it...

A. What I'm saying is, I think he has got the
30 data to do a calculation that would satisfy me, and when Richard Lemen was here I asked him what he thought about this and he said,

A. (cont'd.) well, why don't you write to Dement and talk to him.

When the mail strike is over, I will request that he do that, because I think his is potentially a very important study. I just don't think that his...well, he has not satisfied me with his dose-response calculation. I would like to see him redo it and see what he comes up with.

Q. But to analogize to your study, under his method, his workers would continue to accumulate dose for as long as they worked?

A. Yes.

Q. And correspondingly, as they accumulated dose they may cross the line from exposure group A, to exposure group B, to exposure group C?

A. That's right. The problem is, they can only die in one box, but they are contributing risk to several boxes. I think that's a problem.

Q. So that if we look at...and I don't know whether you were here for Dr. Acheson...but he gave some evidence as to the studies that he used...he and his colleague used...to produce a quantitative risk assessment in England. From what I understand your evidence to be of the four studies he used, the only one that you would consider suitable would be Peto's study at Rochdale?

A. Yes.

DR. DUPRE: Could I just understand what counsel has been asking, in the following way? If, as you believe, Dr. Dement's method leads him to underestimate risk at low exposure and overestimate risk at high exposure, does this mean that in your view his dose-response curve once he corrected for this bias would be somewhat flatter?

THE WITNESS: I have no idea what it would look like. I would be very interested to see, because there is so

THE WITNESS: (cont'd.) little information in the world I think it would be important to see what it looked like.

What I am telling you is that I think his curve...
5 the curve that we have seen in his paper is biased to tilt it up at the far end and down at the zero end. I have no idea what it would look like if he recalculated it.

DR. DUPRE: He does have a curve in his paper?

THE WITNESS: He has a dose-response curve, yes.

DR. UFFEN: Counsel, could I raise something that
10 has been troubling me for the last few minutes?

MR. LASKIN: Sure.

DR. UFFEN: It may be very simple, but I can't see what I have done wrong, or I don't understand.

Would you mind going back to your risk
15 calculations, the example of the lung cancer?

THE WITNESS: Yes.

DR. UFFEN: I think it's on page twenty of tab five, and the bottom half of the page where you work your way down through an example, found the value for the constant C equals seven, then moved down to what the rate would be for one
20 fiber per c.c., rather than the six?

THE WITNESS: Yes.

DR. UFFEN: Divide it by six and you get that long expression, R is equal to the brackets, etc.?

THE WITNESS: Yes.

DR. UFFEN: I can't get from there to the next
25 line where you get eleven plus sixteen.

THE WITNESS: Right. Okay, what you have to do is multiply by the person years at risk, which is ten thousand for the ages forty-five to fifty-four, and five thousand for the ages fifty-five to sixty.

DR. UFFEN: What was that again? Ten thousand...?

THE WITNESS: Ten thousand for the first ten
30

THE WITNESS: (cont'd.) years, and five thousand for the last five years in the total of fifteen.

MR. LASKIN: I might be able to help, because I tried to calculate this myself last night.

But the figure you get for the first category... that is, the figure I get in any event, for the men aged forty-five to fifty-four, the R figure, is one point zero eight, and if you take that figure and multiply it by the person years at risk in the age category forty-five to fifty-four, which for a thousand people will be a thousand times ten or ten thousand, you then get eleven deaths.

Is that...that's right?

THE WITNESS: Yes.

MR. LASKIN: Then if you do the same calculation for the second group, the R figure is three point two four in my calculations, and you will then have five thousand man years at risk, which is a thousand times five, and you get sixteen deaths.

DR. UFFEN: I was apparently multiplying by a thousand for both of them. I missed the point that it's ten thousand for one and five for the other.

THE WITNESS: Correct. You have to take the years of followup into account.

DR. UFFEN: I was doing this little exercise for another purpose. I asked myself the question, how much out, under or overestimate, would you have to make in the doses so that the resulting calculation led you to a number of lung cancer cases virtually identical with the Ontario ordinary, and I got bogged down in it.

But have you done that one?

THE WITNESS: No, I have not.

DR. UFFEN: It looks to me as though it doesn't have to be very much bigger than ten.

THE WITNESS: Well, actually...yes, if I've underestimated by ten, then you get fourteen lung cancer cases compared with twelve and a half. So I suppose if I've underestimated by twelve or thirteen...

DR. UFFEN: Yes, it looks to be about twelve, if you did. So the point I'm making is that it would look as though if the underestimates were of the order of twelve times, then the conclusion would be that there is nothing different from the ordinary population.

THE WITNESS: If current exposures were a hundred and twenty times lower than they were...?

DR. UFFEN: Yes.

THE WITNESS: That's right.

DR. UFFEN: Thanks.

MR. LASKIN: Thanks, Dr. Uffen.

I've exhausted my questions on that part, but are there any...I take it you have a few more comments that you want to make?

THE WITNESS: A. Yes, I just wanted to make a few brief comments about what we can do, if anything, in the future...if I can find the appropriate page.

Okay, as you know, Johns-Manville has terminated their asbestos operations in Scarborough, so workers there will no longer be accumulating asbestos exposure, so that the problem of removal from exposure does not arise in this particular situation.

Are there any other actions that we could take in the future either concerning these workers or others in Ontario, from a public health point of view?

I have listed those on the very last page of this package of handouts, which is labelled Options for Future Action, and I think the most important thing is the item I've called Basic Principle, at the top of the page, which is that any action that either the government contemplates or the Commission

THE WITNESS: (cont'd.) recommends should have built-in to it the principle that we must do less harm than good. I think we have to guard against alarming people without offering them any positive benefits.

At this point in time, I believe that the only useful medical action is periodic examinations, at perhaps three to five year intervals, if a person is believed to be at risk of compensable chest disease on the basis of a risk analysis. Mass surveys of people at low risk are not likely to be fruitful, will needlessly expose people to radiation and may create undue alarm.

I do not believe that surveillance programs for the early detection of cancer among former employees should be undertaken at this time. Neither x-ray screening nor sputum cytology programs have yet been shown to reduce mortality in screened populations. There is no doubt that cytology can detect very early cancers, but has not yet been demonstrated that this influences the ultimate outcome in screened populations.

I believe that epidemiological followup should continue, particularly in connection with case finding and financial compensation. This is probably the most useful activity available to us.

Lastly, I would just like to mention a couple of other studies that we are involved with. As you know, the Ministry is carrying out a mortality study among former employees of the Bendix Automotive Factory in Windsor. We are just about to begin our local trace in Windsor, and depending upon the success of our tracing efforts, results of this study might be available in six to eighteen months.

A second factory of interest is the Raybestos Manhattan plant in Peterborough, where exposures have been solely to chrysotile asbestos. We are negotiating with management at head office for approval to carry out a mortality study at this

THE WITNESS: (cont'd.) plant, but agreement has not been reached.

I am unable to predict if or when we will be able to get started with the study at that plant.

That concludes my prepared remarks.

MR. LASKIN: Q. I'm not going to detain you...I've detained you so long, Dr. Finkelstein, I'm not going to detain you any longer, except I did want to ask you one question that I had forgotten to ask you, and that is whether in connection with your Johns-Manville study you or any of your colleagues are looking at any of the domestic or household contacts of the employees?

THE WITNESS: A. This was a question that I actually raised next to the elevator with the counsel for the Energy and Chemical Workers Union yesterday. The question, the problem has to do with the basic principle, which is to do less harm than good. It's not apparent to me at this point that we will be benefiting wives or family members by subjecting them to a program. We will probably create some concern and it's not apparent yet that we will have anything to offer them from a beneficial point of view in return.

So I'm hoping to get together with some interested parties to discuss the philosophical question of whether it would be worthwhile to initiate a study. That's where it stands at the moment.

Q. I guess the only other final question that I wanted to ask, and I may be asking it too generally for you to answer, but can you, from your knowledge of all the other epidemiological studies that have been carried out and with all of their shortcomings and qualifications and caveats and so on, can you place your own study in the context of those other studies and give us some general assessment as to whether your findings indicate a situation that's more severe at Johns-Manville than,

Q. (cont'd.) you know, any of the other plants that have been studied?

I realize I'm putting it very, very generally, but can you give us any sense of that?

A. I have a tendency to put my foot in my mouth. I would rather reserve comment on that.

I think there is no question that certainly the group of workers I've looked at so far have had an experience which rates among the worst in the world. Beyond that, I don't think...I think I should keep my mouth closed on that.

MR. LASKIN: Thank you very much for being so patient with me, Dr. Finkelstein.

DR. DUPRE: Shall we take a ten minute break or so, and I will assume that your colleagues will give me a batting order.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: Dr. Finkelstein, before I invite the parties to put such questions as they wish to put to you, I would simply like to say that we ourselves will, not least in deference to the time, not have any particular questions for Commissioners' Hour.

What instead we would very much like to do is to invite you to a return engagement with us at some mutually agreeable date, and in the meantime I would simply like to say that we, of course, very much appreciate the importance of this study because it was conducted within this jurisdiction. We appreciate, of course, as well the timing with which it has been submitted to us.

Of course, the flip side of the timeliness, as we all realize, is that the study hasn't had the advantage of peer

DR. DUPRE: (cont'd.) review. To the extent possible, and bearing in mind that this study is now part of the evidence before us, we would hope, to the extent that is possible, to ask some of the experts, the other experts who will be coming before us, to the extent that they have time to assimilate it, to answer a few questions and simply just give us the benefit of their views.

THE WITNESS: I might add for your information, I've sent copies of the asbestosis paper to Margaret Becklake, to Geoffrey Berry and to Julian Peto. When Hans Weill was here, he was given copies of both. David Muir has seen them both.

Unfortunately, the only comments I have received have been from Geoff Berry. I asked Julian Peto for an opinion and the mail strike may have intercepted it, if he has chosen to write one.

So the papers have been distributed, comments have not been received in any great number.

DR. DUPRE: We may express the hope, perhaps, that even if the mail strike interceded that Mr. Peto may have the chance to assimilate it so as to give us the benefit of at least some preliminary views on his second day with us.

So let me just say then that this is the way we propose to proceed, and parties in putting their questions in the time that's left to us might bear in mind that there will be, of course, another opportunity to put questions to Dr. Finkelstein, and for that matter there may be opportunities, to the extent that some of the future witnesses are available, to get the benefits of their views on what we have before us.

THE WITNESS: Might I ask in what context you would like me to return?

DR. DUPRE: Well, presumably in exactly the same context in which you appeared before us today.

THE WITNESS: That would be to answer your questions,

THE WITNESS: (cont'd.) or to present you with additional material, or what?

5 DR. DUPRE: Well, Dr. Finkelstein, I think for the moment I'm going to assume that if you have any, at such time as you come back, if you have any additional material that you think is worthy of our attention, you would consider placing it before us.

THE WITNESS: Certainly.

10 DR. DUPRE: It's understood then, and agreeable?

MR. HARDY: Fine. Just one question, whether the Commission has any idea of the timing of this return engagement. Are we planning on something shortly, or may it well be several months?

15 DR. DUPRE: At the moment, counsel, I guess that one of the things that the Commission remains to be occupied about is that while we are holding days in August, it is not yet exactly clear as to for whom we are holding the appointed days, and in this kind of very...because August is obviously in a very preliminary state, I just can't imagine what the most advisable time would be.

20 I can think of some personal preferences I might have with the situation. As you appreciate, our hope has been to make as full inroads as possible into the whole health effects question by the end of the summer, but chairmens' plans gang aft aglay.

25 Then do I have a batting order?

MR. HARDY: I'm first. That's at least where we'll start.

DR. DUPRE: Please proceed, Mr. Hardy.

CROSS-EXAMINATION BY MR. HARDY

30 Q. Dr. Finkelstein, I think I would first like to clarify some issues with respect to the exposure estimates

Q. (cont'd.) which you made, which are identical exposure estimates for the two papers, the mortality study and the morbidity study.

5 I think you have, yourself, both in the paper and in your testimony today, indicated, for a variety of reasons, the estimates are, as you call them, "imperfect, crude guesses", and you use a variety of different adjectives at different times.

A. Yes.

10 Q. I wanted to explore a few possible areas which perhaps help explain why you feel there are such great uncertainties with the exposure numbers.

I guess first there is the fact that prior to 1969, you have very little hard data to rely on?

15 A. Prior to 1969, I have no membrane filter data at all. I have impinger data from 1961 to 1969, from company sources, some data from government sources, and some insurance company data from the 1950's.

20 Q. And I think from your description this morning, that data, though it covers a period of twenty-plus years in the plant, was in fact recorded on maybe a grand total of five, six, seven days during those twenty years. Is that right?

A. Yes, that's true.

Q. And that data is all area sampling, as I understand it?

25 A. Yes, with one caveat. The company, in doing their sampling, had the impingers on some wheeled vehicles, I guess, and they kind of attempted to keep the sampling nozzle in the worker breathing zone. So it's not an area sample in the sense that the Rochdale samples are area samples. They are not fixed pipes, or whatever, sitting at various places. The company hygienists realized that there was worker mobility, attempted to
30 place the sampler in the worker breathing zone, so it's a hybrid kind of thing. It's personal area impinger sampling.

Q. Is that true for all of the samples from 1948 through 1968?

5 A. It's true for all of the company samples starting 1961. I have no idea what the insurance company did in 1954-1957. The Ministry, I believe, just set up a tripod in the region of the lathe operator, or between pipe rolling machines, or something of that nature.

10 DR. MUSTARD: Could you give a clarification of how frequently the company sampled once it started sampling in 1961?

15 THE WITNESS: The company sampled at least annually. If there were problem areas or if they were concerned, they went back and did it several times during the year. But there was an annual program which may have been done more frequently in certain areas if they were concerned about conditions.

MR. HARDY: Q. That began in 1961?

THE WITNESS: A. That began in 1961. In 1969, they began quarterly personal membrane filter sampling.

20 Q. The baseline measurements that you use to do your exposure calculations are personal samples done with the membrane filter method?

A. Yes.

Q. In 19...?

A. In the autumn/winter of 1969, and spring and early summer of 1970.

25 Q. Do you know what sort of counting method was used for those samples?

A. I don't. Ed Stevens, the company hygienist, told me that this was the technique that they were taught at Manville, and which presumably is the technique that is still currently used by the company.

30 Q. That's your assumption? You are not sure whether it was or was not the same technique?

A. That's absolutely correct. You'll have to ask Mr. Stevens about that.

Q. So you don't know whether there would be a correction factor as has been discussed in the English plants, between full field and graticule counting?

A. That's right. I don't know. I suspect that the American and the British techniques were somewhat different, and the American technique has not changed much, if at all. But I have no personal knowledge of that.

Q. In comparing, in assessing what you calculated to be the exposures pre-1969, you make several assumptions, and the first assumption is that the dustiness from job to job stayed relatively constant?

A. Yes.

Q. I gather you had to make that assumption because you really don't have enough data pre-1969, on a job-by-job basis, in order to calculate the exposures for each job over the whole period?

A. No, that's not true. I could have done what they did at Rochdale, which was take ratios of things changing in time. I decided that since my uncertainties were so considerable that this would be mere window dressing, and I decided just to go ahead and make the assumption that I made.

I could actually have taken ratios year by year for each of the various jobs. I think that's unrealistic...or I thought that was unrealistic and I chose not to do it.

Q. But it is possible then that your assumptions would obscure some job stations which historically...at which historically there have been more than average improvements?

A. Yes.

Q. In exposures?

A. Yes.

Q. Which...would that conversely mean job

Q. (cont'd.) stations where the exposures had been even higher in the earlier years...?

5 A. That's true, or the converse is true, they may have been even lower.

Q. I gather, you do indicate in your paper that your assumption about relative exposures among stations is, as you say, weakly supported by the evidence. Did you do any statistical checks to see how much it's supported, or do you prefer just to use that qualitative description?

10 A. I prefer to use the qualitative description. The data comes from a variety of sources which may or may not be very comparable. I think Hans Weill in the quote that I've given you sums it up quite nicely. One hopes there is comparability, we assume there is comparability, but that's all we can do.

15 Q. And your other two assumptions with respect to exposures pre-1969, are that the 1969 figures should be multiplied by thirty percent for the period from 1954...

A. 1955 to 1962.

20 Q. And by a factor of two for the period before that, back to the beginning of the plant in 1948.

I gather you get those rough numbers by looking at what limited exposure data there is in those periods?

25 A. Partly on limited exposure data, partly based on conversation with company employees...in particular two industrial engineers, Mr. Shelvington, who as I mentioned, designed the ventilation system in 1970 and was present in the transite pipe plant as a general worker in the early fifties, and Mr. Mulholland, who is currently at head office in Denver and who was present from the time the excavation was begun in 1947.

30 Mr. Shelvington unpromptedly volunteered the opinion that perhaps conditions were twice as dusty in the bad old days. Mr. Mulholland, I directly asked the question phrasing it as, I

A. (cont'd.) am proposing to you a factor of two, how does this strike you. And he said it sounded quite reasonable to him, based on his personal knowledge of the plant.

5 Q. He didn't probe then to see whether a factor of three might be just as reasonable, too, given all the uncertainties?

A. I say in my paper the uncertainty of three to five is not unreasonable, based upon this data.

10 Q. You are speaking there in particular...

A. Cumulative exposure.

Q. ...about a factor of three to five greater than the exposures you are talking about here?

A. Well, the presumption is they could be greater. On the other hand, I may have overestimated the exposures.

15 Q. That certainly doesn't seem very likely though, does it, from the limited data you do have?

A. That's a qualitative judgement. I don't know, no one knows. Certainly work practices in the bad old days were not very good. People were, apparently, jumping in bins, dry sweeping, doing all kinds of not very nice things.

20 I think it would be a reasonable assumption that exposures were high. On the other hand, there may have been particular employees where I have overestimated their exposures.

The intention in my statement of three to five was that this was the range at which I may have underestimated.

25 Q. In talking about jumping into bins, dry sweeping, is it not quite possible that those sorts of exposures which would have occurred in such circumstances would not have been recorded in that limited data we have?

30 A. Absolutely, which is why I'm curious as to how the Johns-Manville hygienists assigned their exposures for Drs. Enterline and Weill. I've asked the company for comments on how I did it. They chose not to give me any constructive criticism.

A. (cont'd.) I'm still quite prepared to hear how they did it in the good old days.

5 Q. No constructive criticism? I gather though, from some of your discussions this morning, that you have learned some facts about conditions in this plant from Johns-Manville, which are reflected both in the writing here and in things you...

A. That's right. I sent...I would prefer not to get into this. If you push me, I'll...

10 Q. Well, I just recall that you did discuss this morning, and you put in the paper, some factors relevant to estimated exposures in this plant.

A. Yes.

Q. In all those cases, you have indicated that that information was provided you by Johns-Manville people.

15 A. Oh, absolutely. Absolutely. This was qualitative information.

Q. Okay.

20 Also not reflected in the sort of area measurements which do exist for the early years, as I gather it, would be the sorts of exposures that might occur on those occasions when the plant closed down in order to clean up or repair operations, which I understand they may well have been very high?

25 A. That's right. I think every remark that you are making is absolutely valid, every remark you will make along these lines for the next few minutes will also be absolutely valid. I think that this study cannot be looked at in isolation. I think it has to be looked at in comparison with what other people in the world have done. I have showed you what they have done, no one else has been able to take this into account. I haven't been able to take it into account. Any other caveat you care to bring up, I will beforehand agree with.

30 Q. That's a great opportunity, Dr. Finkelstein.

A. That's right. If there was equipment breakdown,

A. (cont'd.) and...there were all kinds of things which I haven't been able to assess and neither has anybody else.

5 I make no claims for great accuracy. All I'm attempting to do is to add an additional little bit of data to the complete absence..almost complete absence of knowledge about asbestos risk in the world, and I think this work has to be looked at in that context.

10 Q. Fine. I'm just curious, and I better ask you now before I forget it, you say that other people who have provided exposure information for such studies, as with the case in the Weill and Enterline studies, weren't able to take these factors into account. Are you sure of that, or what you are really saying to us is that you don't know, since they haven't published descriptions, how they took these things into account?

15 A. Well, we certainly know for Peto's study, because he just told us they multiplied by the area sampling data. Enterline says...well, you've got the quote in front of you, I haven't got it in front of me...but in essence it was impossible to do it, so we just sort of roughly classified them.

20 That was done by Johns-Manville's hygienists, I presume.

Q. And you don't know to what extent those hygienists took into account factors like peak exposures, maintenance breakdowns, six-day weeks, changes in work practices?

25 A. I've seen all the raw data that exist for this plant. I don't see how I could do it, and I don't see how even the most informed and educated Johns-Manville hygienists could do it either.

I really don't know what they've done. There is no published description.

30 Weill makes no attempt at any description at all. He just says, they handed me the data.

A. (cont'd.) So if you can find out for me how they did it, I would be very interested to know.

Q. I guess the point I'm trying to make sure that we have clear is, you are saying you don't know how they did it?

A. That's right.

Q. You do know how you did yours?

A. That's right.

Q. So at some point maybe we'll find out how both of them were done, and we can compare them.

A. I might say that the company knows exactly what I've done, because I've sent them drafts of all my plans and asked for comments. In return, they didn't tell me how they did it.

Q. You mean when they did it for the Enterline study?

A. Yes.

Q. Many years ago?

A. Yes.

Q. Whoever it was at Johns-Manville at that time who did it?

A. That's right.

Q. One other factor, with respect to the exposures at least, that strikes me that maybe we should bring out, has to do with what would have been shown over the years had there been personal samples available for all the workers. I gather that the two major changes which are reflected in your estimates and assumptions are when general ventilation was upgraded in the plant, which occurred twice during the period?

A. That is one factor. The other factor is local ventilation changes, of course.

Q. That's right, and the local ventilation changes are not figured into your assumptions, is that true?

A. They would probably be reflected by the company

A. (cont'd.) impinger data throughout the sixties.

5 As a matter of fact, there are comments by the hygienists, such-and-such widget on machine X modified, and then they would go back and they would check.

Q. And improvements would be found?

A. There is no great indication of substantial changes in the impinger data.

10 Q. Which may or may not have been reflected in changes in personal monitoring?

A. Absolutely correct.

15 Q. So to the extent there were those sorts of personal ventilation changes throughout the period from 1948 to 1970, they are probably not reflected in the estimates and assumptions you have made?

A. If they bore no impact upon area monitoring, that would be true.

20 Q. Maybe we should now turn to your asbestosis study, which is tab four, and talk about a couple of factors with respect to that study.

The outcome which you measure in that study is certified asbestosis?

A. Yes.

25 Q. I gather for purposes of this study that has a particular meaning to that term?

30 A. Yes. Well, actually, that's what Berry and the British call it. In Ontario we would call it compensated asbestosis. I treat those as synonymous because I am comparing my work with Berry's, I adopt his terms. But these are men who are receiving disability pensions from the Ontario Workmen's Compensation Board.

Q. Is that a term which has had a constant meaning over time?

A. The term has had a constant meaning, yes.

Q. In other words, you are telling me that the same standards were applied by the Board throughout the period we are concerned with here to determine whether or not a man should get compensation?

A. No, I'm not telling you that. I'm telling you that the name was the same.

Q. Oh, have the standards for compensation changed?

A. I have never been present at an examination of one of these workers. I have no idea of the precise criteria that have been applied to any individual worker in this study.

Dr. Muir, who is a consultant to the Commission, I believe, is a member of this committee, and he might be able to shed some light in his recent experience.

I wouldn't be surprised if the criteria have changed with time, as the membership of the committee is changed with time.

Q. In which case you would be possibly talking about different conditions in the men you consider over time?

A. Well, I think the condition is probably the same. What may have changed is the percentage assessment of disability, which may vary from committee to committee.

That didn't enter at all into my considerations. As soon as a man was declared disabled and pensioned, he entered the study regardless of whether this was considered to be a ten percent disability or a hundred percent disability.

Q. But you in fact did not go behind the certification of disability, whatever...

A. That's correct.

Q. ...to determine whether in fact the conditions for all the men were...

A. That's correct. I looked at a list of men, in essence, and these are men who are or have been pensioned

A. (cont'd.) by the Ontario Workmen's Compensation Board.

5 Q. I know you discussed this in the paper, although you didn't mention it this morning, you similarly do not really know to what extent disability awards in Ontario equate to what is called certified asbestosis in England?

A. That's absolutely correct. This is a local provincial criterion decision, and I have absolute no knowledge about what other jurisdictions might do in similar circumstances.

10 Q. So when comparing your results to the Berry results, there is a little bit of problem in not knowing for sure whether you are comparing the same thing because of outcome in his study and outcome in your study?

15 A. Well, since we are comparing labels, we are comparing the same thing. The label is certification for asbestosis - in England by the Pneumoconiosis Review Panel, and in Ontario by the Ontario Workmen's Compensation Board.

20 I will tell you that in England I have read some certified men coming to autopsy have not had asbestosis at autopsy, whereas some men who have never received certification did have asbestosis at autopsy. So there is some uncertainty about the diagnosis.

However, the label is the label, and I compared the labels, in essence.

25 Q. I would like to talk for a second about that comparison between your study and the Berry study, and I believe you described the comparison on pages sixteen and seventeen. This is a comparison with respect to the incidence rates Dr. Berry found and the incidence rates you found.

30 To help understand the questions, it might be useful if you have..and I don't know whether you do...the incidence table that Dr. Berry provided from his study, which among other places is table twenty-two in the Simpson Report.

A. I have not brought it with me, no.

MR. LASKIN: I can put it in front of the witness.

MR. HARDY: Q. Maybe I should just check with the
5 witness to make sure that this is the incidence data from Dr.
Berry which you discussed?

THE WITNESS: A. I used his 1979 British Journal
of Industrial Medicine paper. I have no idea whether this table
is the same.

MR. LASKIN: I can get it for you.

MR. HARDY: Can you get it?

MR. LASKIN: Sure.

MR. HARDY: Just to make sure the figures are the
right figures.

MR. LASKIN: I don't believe it was in the report,
15 because that was one of the studies which Mr. Berry produced
after Dr. Acheson did his report.

THE WITNESS: A. Let me make an unsolicited
comment, which is that these are qualitative comparisons. I
have no intention of saying that he found one point three percent
and I found one point four percent, or I found two point seven
20 percent, and therefore there is a difference.

The sole intention of my comparison was to see
whether there was ballpark agreement between his work and mine.

That was the question I asked myself and I
think I've answered to my satisfaction. If you have particular
questions about whether a number should be point four or point
25 five, or one point three or one point seven, I think that I
personally am not concerned about discrepancies of that
magnitude.

MR. HARDY: Q. Maybe we can do it with this
table. If the numbers look different from the numbers you
30 remember though, we'll go find the other table. I think it's
the same table.

5 Q. (cont'd.) You talk in your paper, on page sixteen, about the three incidence rates found for men first employed after 1950, by Mr. Berry, and those are point four percent for people with between zero and forty-nine fiber years per cubic centimeter, one percent for the next exposure group - fifty to ninety-nine...

A. Mmm-hmm.

10 Q. ...and two percent for the next exposure group of a hundred to a hundred and forty-nine?

A. Correct.

Q. I think those are the numbers which, on table twenty-two in the category B, men first employed after 1950, listed under, as the annual percentage under possible asbestosis?

15 A. That's right. If you look on page sixteen of my paper, fourth line...third line from the bottom...I say they observed incidence rates of possible asbestosis of zero point four percent per annum.

Q. Okay. So we are just confirming that this table probably has the right numbers?

A. Right.

20 Q. Okay. The first question I have is why you compared your numbers, your incidence rates, to the Berry numbers for possible asbestosis, as opposed to the Berry numbers in the next column over, which is for certified asbestosis.

So in fact that's the label we were talking about?

25 A. Yes. Well, we'll probably have to check the paper, but I don't imagine certified asbestosis appeared in the British Journal of Industrial Medicine paper.

Q. And therefore...?

A. Therefore I couldn't use them because I was not aware of this particular table in the Simpson Report.

30 Q. Okay. Now seeing it, is it possible perhaps that the certified asbestosis annual percentage numbers may be the

Q. (cont'd.) ones that are more likely to be the ones that should be compared with your incidence numbers?

5 A. Well, you know, I think if we look at their...the less than fifty are about the same...well, they were talking about point four, point one, one versus point nine and two versus one point one. I think there is essentially no difference between those numbers, for all epidemiological purposes.

10 Q. Similarly, I was curious why you focussed attention in your comparison on various groups of men employed first after 1950, rather than looking at the data he supplies up above for men first employed before 1951, who I would gather he had much longer latency periods and followup on?

A. Again I refer you to the published paper, which talks about men first employed after 1951.

15 Q. So that I guess the question then would be, would not it also be relevant in doing this comparison to look at what Mr. Berry found for this group first employed before 1951, for whom you had probably as long a followup as you have in your studies?

20 A. Yeah, well you see the problem with all that is he doesn't know what happened to these people. They have all disappeared from view. You know, he quite clearly states that the 1966 or 1968 British Occupational Hygiene paper has major flaws because it was a cross-sectional study and there were some short-term people there. The only people they invited to come back were the people hired after 1951.

25 Therefore, all these other guys either died...well, I don't know what happened to them and neither does he. He therefore confined his analysis, his subsequent followup analysis, to the 1951 group for two reasons. One, these were...1951 was the first year that an environmental assessment was made in
30 the plant, and (b), these are the people he invited to come back and as you know from reading his papers, he found that they

5 A. (cont'd.) grossly underestimated the risk in their first paper, because it was a cross-sectional study. When they invited these people to come back, they found that the incidence rates of asbestosis had markedly increased.

If he had invited his pre-1951 people to come back, I'm sure he would have found the same thing.

10 Q. I'm not sure what my understanding or your understanding is of to what extent men first employed before 1951 were included in this table twenty-two, and I gather you are making some assumptions about the tracing rate in that group, is that right?

15 A. No. If you read his paper, he tells you what he has done. I haven't memorized it, and I haven't got it in front of me, so I can't tell you precisely, but they limited themselves in their followup 1979 paper to the post-1951 group.

20 Q. But they also report here in table twenty-two some data on the pre-1951 group, with some incidence numbers calculated. I certainly don't recall, and I'm not sure whether you know for sure, what sort of tracing was done to get the data in this table...this is the table he did in 1979. It's not the old 1968 study.

25 A. Well, you know, I can't comment on this. The only thing I could comment on is his published British Journal of Industrial Medicine paper and the paper he coauthored with Lewinsohn in the Annals of the New York Academy of Sciences, 1978, 1979, whenever that was, in which they limit their quantitative analysis to the post-1951 group.

30 Q. Let me ask one final question about the comparison between your results and the Berry results. Are you aware of the fact that Mr. Peto, who will be here tomorrow, has recently indicated that there has been some recalculation of the exposure numbers for the Rochdale plant, which will be the workers we are talking about here?

A. Yes, I am.

Q. As I understand it, the recalculation indicates that in the 1950's exposures may have been three to five times higher than previously assumed. Is that your understanding?

A. I'm not sure what they are doing, quite frankly. It's being done by company hygienists. As you may recall, the data from the 1950's was virtually nonexistent. There was thermal precipitator measurements from 1951 or 1952, and some thermal precipitator measurements from 1960.

I am not quite sure how one can apply correction factors to what is probably extremely unreliable data to start off with.

Q. It is true that Mr. Peto has come up with a new table which he presented in Lyon, which I think we have looked at here previously?

A. Yes. His table looks at a small group of the total men hired from 1951 to 1956, and this is preliminary sort of data.

Again, he isn't doing the calculations and I've no idea what he thinks about them.

Q. Okay. But it is true, I think...and I think we went through this with Mr. Berry when he was here, that these incidence calculations that we are looking at in table twenty-two were based on the old exposure numbers?

A. Yes, that's right.

Q. To the extent that the best thinking...whatever that may be...about the exposure numbers at Rochdale now have higher exposure numbers, this table would show lower incidence rates for each category of exposures?

A. Yes, and I might point out that the sampling techniques here we are talking about are completely different. Mine are personal membrane filter sampling, theirs are area sampling by thermal precipitator, whatever that is.

A. No, I do it...the basic premise is different. I set a...after thirty-two years based on this period of observation, this is what one might see.

5 The British extrapolate to a working lifetime of fifty years, based on a maximum followup of twenty-three years. Given the Ontario experience that most cases of asbestosis occur after twenty years, or after his maximum followup interval, I don't think these numbers are directly comparable.

10 Q. Okay. I guess the first question I have is, if in fact your exposure numbers are too low by a factor of three to five, I assume that means that the one percent that you would calculate on this chart would occur as a considerably higher exposure level than ten? Would it be thirty and fifty, or have I oversimplified?

15 A. Yes. I calculated that number because they calculated a number, and I had an equation and it was fun to plug in numbers.

20 I don't think that one can make very accurate quantitative deductions from this kind of thing. It wouldn't surprise me if, you know, exposures to the current levels of a fiber or two will prevent most of the work force from developing asbestosis.

 As I have indicated, however, I don't think asbestosis will be the major problem in the future, but cancer will be.

25 Therefore, we can play numerical games with this data, but I don't think it really serves much of a purpose because I personally wouldn't use it for risk assessment in future circumstances.

30 Q. I gather if current plans in their attempt to meet two fiber standards in fact are having average exposures in the plant, say, in the order of point two or point three, point five fibers, you would feel even more strongly that at those

Q. (cont'd.) even lower exposure levels worrying about asbestosis for occupational standard setting would probably not be a fruitful place to concentrate your attention?

5 A. Well, there are two things I can say. First of all, I think Johns-Manville, before they shut down, set an excellent example for the rest of the asbestos industry in Ontario.

They were routinely bettering half a fiber, both government and company hygienists were routinely measuring exposures of point one to point three fibers.

10 So I think this is an example which stands out in Ontario and which I think other industries and factories should attempt to emulate.

So that's that part of it.

15 As for whether we should be worried about it or not, the previous witness...I don't remember who...quoted numerous optimistic comments that were made in 1910 by Dr. Murray, who described the first case of asbestosis, about the British in 1931, about other people saying that everything is now perfectly okay, we've got no problems in the future.

20 I cannot be superconfident that we won't be falling into the same trap now. It would appear, based on data like this, that average levels of point two, point five, will probably prevent clinical asbestosis from occurring in the vast majority of the work force.

But I could be invited to come back in forty years and say that I was wrong.

25 Q. Something you would find out if you are wrong, which you may well not be, because there would be more accumulated data on workers at those sorts of exposure levels?

30 A. I am saying it would appear that those levels will not produce clinical asbestosis in much of the work force, but it might.

MR. HARDY: I have some questions on the mortality study, Mr. Chairman, and also on the most recent handouts that we've got from Dr. Finkelstein. Given that he is coming back, and other people might want to ask questions, I would be willing to reserve them or I would be willing to the ask them, at the pleasure of the Commission.

THE WITNESS: I would be prepared to stay as long as the Commission would like me to.

DR. DUPRE: Well, what are the timing...what's the timing outlook for the other parties?

MR. LASKIN: Maybe I could just have a brief discussion with...

DR. DUPRE: Would you like to put your heads together?

MR. LASKIN:...for about five minutes and I'll tell you what we are going to do.

DR. DUPRE: If I can help, we can bend the six o'clock adjournment say to about six-fifteen to accommodate everyone, but I don't think the Commission can go beyond that.

MR. LASKIN: Well, with all the caveats that lawyers always put on their time estimates, and there are many, I think the parties are at least going to be another hour and a half, an hour and fifteen minutes to an hour and a half, and I don't know about M. Casgrain. He's somewhere in the building.

We certainly can't curtail cross-examination. I am really...

DR. DUPRE: So what you are telling us, counsel, is what? Are you telling us we have to stay until seven, or are you telling us that we should simply pursue this at such time as we reconvene?

MR. LASKIN: I guess I'm really telling you, Mr. Chairman, that in view of the long day that all of the three of you have had, and the witness, I think it's really in the hands

MR. LASKIN: (cont'd.) of the three of you and the witness just how long you want to stay. All I can tell you is the time estimates that we've got.

5 THE WITNESS: My personal preference would be to get as much of this over with as possible tonight.

DR. DUPRE: Counsel, the Commission is bearing a number of things in mind at this point, including its own need to prepare for a most important witness who comes from the U.K. for only a short period of time, tomorrow.

10 So I think that for the moment we might be well advised to perhaps almost call it a day at this point, and possibly schedule the immediate question that counsel still legitimately, the parties, I am sure will want to ask, perhaps as soon as possible, not to confuse that particular occasion, if you know what I mean, with the one that I mentioned at the
15 outset.

In other words, to translate this Latin into Greek...

MR. LASKIN: I think I understand.

20 DR. DUPRE: ...I put this to you: Supposing by some chance Mr. Peto is finished by noon, say, on the second day, it might be then possible to put an hour and a half up front, say from two to three-thirty, for Dr. Finkelstein, if it's possible for him to come back.

MR. LASKIN: If he's available.

25 I think that's a very sensible suggestion, Mr. Chairman, speaking for myself.

DR. DUPRE: And if a July 30th thing doesn't work, well then again we could try to slot in, again given Dr. Finkelstein's availability, an hour and a half whenever it is - the 14th of August, or the 15th, and so on.

30 Shall we...

MR. LASKIN: Is that agreeable to everyone?

DR. DUPRE: Is what I propose clearly understood?

Dr. Finkelstein, you are agreeable to this?

THE WITNESS: Yes, sir.

DR. DUPRE: Fine.

Well, thank you very much indeed, and may I say,
Dr. Finkelstein, that we are very grateful for your appearance
here today, and among other things I should say that on a team of
very distinguished visiting professors, you are peerless in terms
of your capacity to explain the subject.

THE WITNESS: Thank you.

DR. DUPRE: We very much appreciate your comments.

Thank you.

We stand adjourned then, until ten o'clock tomorrow
morning.

THE INQUIRY ADJOURNED

THE FOREGOING WAS PREPARED
FROM THE TAPED RECORDINGS
OF THE INQUIRY PROCEEDINGS

Edwina Macht
EDWINA MACHT

